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THE STUDY OF EYE MOVEMENTS IN READING

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The study of eye movements continues to be an important and much used technique for investigating the reading process. An overall summary of the field up to 1935 is given by Tinker (108). The last review (107) to appear in this journal was published in 1936. In the present review, material appearing during the years of 1935 through 1944 is considered. Bibliographies, critical evaluations, summaries of parts of the field, and textbook discussions will be found in references (13, 37, 38, 41, 74, 95, 97, 103, 116, 117). A few titles not available to the reviewer have been included in the bibliography for the sake of completeness (6, 29, 30, 46, 47, 48, 49, 126). Certain material on eye movements will not be reviewed since it is not closely related to the reading process. This includes reports on eye movements in viewing pictures and advertisements, certain studies of visual fixation, eye movements as related to visual attention, and the like.

The studies to be reviewed here group themselves as follows: techniques of measurement, analysis of the reading process, oculomotor coordination, factors in variability, special reading situations, heredity and certain environmental factors, eye movements in the reading clinic, training eye movements, and central versus peripheral processes.

Techniques of Measurement

Apparatus. Although some of the new apparatus for recording eye movements consists of modifications of earlier techniques, others involve new principles. Brandt (9) describes an adaptation of the Dodge corneal reflection type of camera. Its special feature is simultaneous recording of the vertical and horizontal components of eye movements. A portable model of the Dodge type of camera, the Ophthalm-o-graph, is described by Taylor (105). Records of horizontal eye movements of both eyes are recorded simultaneously. Tiffin and Fairbanks (106) have developed a camera for simultaneously recording eye movements and voice reactions in oral reading. Miles' (70) note on the first eye-move-

ment record photographed by Dodge has historical interest. A demonstration film on eye movements in reading has been prepared by Valentine, Troyer and Brown (118).

An electrical method of recording eye movements is finding some use. Changes in the corneo-retinal potentials are closely proportional to the sine of the angle of rotation of the eye. With proper placement of electrodes and with an amplifying system, the movements may be recorded mechanically or photographically. Miles (71) and Halstead (40) suggest that eye movements in a variety of situations may be measured by this technique. Its validity for recording eye movements in reading has been checked by Hoffman, Wellman and Carmichael (43). Electrical and corneal reflection records were made simultaneously while their subjects read and while they made horizontal and vertical eye movements in viewing a dot pattern. The only data given for the reading responses was a sample record. Although the two techniques yielded similar records, they were far from identical. Where extent of eye movements and duration of fixation pauses for viewing the dot patterns were measured, the agreement as shown by correlations is close but again the records do not yield identical results. Although this electrical technique has sufficient validity to justify its use in certain situations where it is impossible or inconvenient to photograph the corneal reflection, it is clear to the reviewer that it is not satisfactory in its present form for studying eye movements in reading.

Another electrical technique, recording photographically (electromyograms) the action currents generated by movement of extrinsic eye muscles during reading, has been used by Luckiesh (61) and Luckiesh and Moss (62, 63, 64, 65). The sample record given by Luckiesh (61, p. 128) is either not typical or the author has read the time intervals wrongly. He gives 0.120 sec. for time of back sweep when the typical value is approximately 0.050 sec. The duration of his interfixation movement also appears unduly long. Furthermore, the demarkation between successive fixation pauses is not always clear. A direct comparison of this technique with the corneal reflection method is needed to check the validity of the former. The corneal reflection technique of photographically recording eye movements, therefore, remains the most satisfactory method for use in reading investigations.

Reliability. The reliability or consistency of eye-movement records has been considered by several workers. Imus, Rothney and Bear (44), using fifty word samples of text, found that eye-movement measures had low reliability ($r = .59$ to $.72$). In another report Imus, Rothney and Bear (45) re-emphasize the unreliability of eye-movement records.

Employing the same material, i.e., that furnished with the Ophthalm-o-graph, Anderson (3), and Broom (12) note similar trends in low reliability. In a more extensive study Tinker (112) found that reliability of eye-movement measures was low only when short samples of text (5 to 15 lines) were employed. When 20 to 40 lines were read the reliabilities were about .80. Thus eye-movement records will have adequate reliability only when a satisfactorily long sample of reading is used. When only group comparisons are involved, short samples are adequate. But in the clinic, where individual diagnosis occurs, records from reading at least 20 lines of text are indicated. An even longer sample is desirable.

Validity. If eye-movement measures are to have meaning for validity evaluation there must be adequate comprehension of the material read. Users of the Ophthalm-o-graph for photographing eye movements have commonly employed short samples of reading material with questions on comprehension furnished with the apparatus. Imus, Rothney and Bear (45) have found this check on comprehension to be useless. Authors such as Broom (12), Anderson (1, 3) and Imus, Rothney and Bear (44, 45), have reported very low validity coefficients for eye-movement measures of reading when correlating eye-movement measures with scores on reading tests. Such a comparison constitutes an unsatisfactory test of validity since it is well known that reading scores derived from different reading situations are not related or yield only low correlations. Tinker (112) has shown that when the material read before the camera is strictly comparable to that in the performance tests the validity of fixation frequency and perception time (fixation frequency times pause duration) was very high ($r = -.80$ to $-.99$). Pause duration by itself was found not to be a valid measure of reading performance.

Other evaluation. Is a reader able to give a typical reading performance in the apparently artificial laboratory situation where the photographing of eye movements takes place? Tinker (112) has checked this and found that the performance was the same before the camera as when working at a table. This finding has been confirmed by Gilbert and Gilbert (34). It should be noted, however, that it is important to adapt a reader to the laboratory situation when photographing eye movements.

Stromberg (101) has questioned the common practice of plotting the fixation points of the eyes to show the location of these fixations on the material read. This has ordinarily been done for a single eye, assuming that the two eyes coordinate in fixating on the text. When Stromberg

plotted the fixations for both eyes he found what appear to be startling discrepancies in binocular fixation. That is, the fixation of one eye frequently did not coincide with that of the other eye. Undoubtedly there has been unwarranted optimism concerning the usefulness of plotted fixations to show exactly where the eyes paused. Since one fixates an area rather than a point, and because movement of the reflected beam does not correspond exactly to the visual angle of movement, plotted fixations can only represent approximations to the correct place of fixation. One suspects that part of the binocular fixation discrepancy found by Stromberg was due to the reflected spot of light not being in corresponding locations on the two eyeballs, a condition which would prevent correspondence on the photographic record. Nevertheless, this study does raise an important question concerning interpretation of plotted fixations.

In general the evidence indicates that under adequate experimental conditions, eye-movement records are reliable and valid measures of reading performance.

Analysis of the Reading Process

The fixational pause. Perception occurs during fixational pauses. Arnold and Tinker (5) made an extensive analysis of the fixational pause of the eyes. On the average a pause of .157 sec. was required to identify a letter and .172 sec. to accurately fixate a dot. These are longer than the .100 sec. required for a well cleared-up perception. In a variety of reading situations the mean pause duration varied from .217 to .404 sec. These are much longer than the pauses made in simple fixations requiring a minimum of perception. The longer durations in reading may be explained in terms of the requirements for comprehension and assimilation of the materials read. Luckiesch and Moss' (65) statement that reading pauses average about .150 sec. is a misrepresentation. Reading with comprehension cannot have a mean pause duration as short as this.

An excellent analysis of regressive pauses, i.e., pauses following backward eye movements within a line of print, in the reading of 9th and 10th grade children is given by Bayle (7). She found that regressive pauses tended to fall into six patterns: adjustment after the first fixation in a line, adjustment within a line when the span of vision is overreached in a too long forward move, regressions for verification, regressions during word analysis, regressions for phrase analysis, and regressions for re-examination of a whole line. It was found that regressions were caused by need to complete perception and by difficulties of

interpretation due to failure to recognize the meaning of a word or failure to connect meaning of a word with the context. Bayle points out that regressive pauses are necessary parts of the reading process in analytical reading. Buswell (14) gives a cautious approval of this view but many writers ignore it. Recognition of the fact that regressions are legitimate and essential in certain kinds of reading would avoid some of the misconceptions involved in training eye movements (see p. 110).

The fixation span. This is the amount of material read in one fixation. The larger the span, the fewer the fixations and consequently the faster the reading. By using reading material with spaces between phrases and encouraging readers to fixate each phrase only once, Robinson (87) obtained the relation between perceptual span from tachistoscopically exposed material with fixation span in reading. For tachistoscopic material not in context this correlation was .52, for material in context (using a special tachistoscope) the correlation was .66. He concluded correctly that span derived from short exposures predicts eye-movement behavior inadequately. La Grone (55) and Knehr (51) also found a low correlation between perceptual span and fixation span. Dearborn and Anderson (27) have perfected a highly flexible and effective motion picture technique for exposing successive phrases in context to increase the fixation span.

In a study of adult reading, Buswell (14) emphasizes that fixation span or span of recognition is an important index of reading maturity, i.e., a narrow span indicates immature reading habits. He found no relation between visual efficiency and span. There is a similar emphasis upon fixation span in the author's second monograph (15). In all of Buswell's work (14, 15) there is a sound emphasis upon training to increase the span of recognition which directs attention to perception rather than to the motor aspects of eye movements.

Luckiesh and Moss (62) have studied the influence of typographical factors on extent of the fixation span. They found that span decreased slightly in going from four to 10 point type and that it increased slightly in going from 13 to 21 and 29 pica line widths. Their interpretations reveal a decided lack of understanding of the reading process. No account is taken of possible accompanying changes in either pause duration or efficiency of peripheral vision.

Oculomotor Coordination

Rhythm. Much has been made of rhythm in eye movements. Buswell (14) states that "a mature reader covers a line of print with regular,

rhythmic eye movements" but admits that reading habits should be flexible and that in some situations regressions are desirable. Taylor (105) has devised techniques for developing "rhythmical, left-to-right eye-movements." In the non-experimental literature the importance of rhythmical eye movements is stressed, i.e., regular forward movements along a line of print and about the same number of fixations from line to line are considered highly desirable. This emphasis is derived from the notion that the efficient reader is efficient because he is able to establish rhythmical eye-movement habits more readily and that favorable typography fosters rhythmical movements. Sisson (94) has made a quantitative analysis of motor habits of eye movements in reading as related to length of the printed line, kind of material read, and level of reading ability. Tendency toward rhythmical motor habits was evaluated by means of a habit index, i.e., the percentage of successive lines having the same or nearly the same number of fixations. The habit index was higher for reading the shorter lines of print and slightly higher for the more efficient readers. This apparent relation is accounted for in terms of probability, however, rather than a habit for the fewer the fixations, the better the chance of two adjacent lines having the same number of fixations. The habit index favored neither the reading of easy narrative nor scientific prose. Another alleged sign of rhythmic eye movements or *short-lined motor habits* as they are sometimes called, is a characteristic temporal distribution of pause durations which is alleged to be fostered by shorter lines. Although some evidence for such a pattern was found, it was no different in short than in long lines of print. In view of his results. Sisson suggests that the concept of *short-lined motor habits* is useless. The reviewer would add that this notion of rhythmic reading is not only a useless concept but a harmful one. For years the view that rhythmical eye-movement behavior is characteristic of effective reading and is highly desirable has directed attention toward eye movements. This has led to an undue emphasis upon peripheral oculomotor mechanics to the sacrifice of adequate attention to the more important central factors of comprehension and assimilation (see *training of eye movements* p. 110). The findings of Walker (120) are in line with the above conclusions. He found "no evidence of a habit of moving the eyes any fixed distance or in any rhythm" in his analysis of eye movements of good readers. If rhythm is absent in the eye movements of highly skilled readers, as indicated in these studies, the view that rhythmical eye-movement patterns are desirable for effective reading becomes meaningless. Weaver (123) found a high degree of

variability in ocular behavior characteristic of both memorizing and recall.

Motor efficiency. Tinker (111) has ascertained the relation of oculomotor efficiency to reading proficiency. There were no significant correlations between reading proficiency and either accuracy of visual fixation measured in terms of number and extent of corrective moves at fixation point or speed of convergence-divergence moves. Only when extremes of the group were compared was there a slight relationship evident. These findings refute previously held views. Similarly, Strombert (99) found no differences between fast and slow readers in convergence and divergence movements during reading. Knehr (51) noted that vergence movements in reading monocularly were the same as for binocular reading.

Eye-movement time. The proportion of time taken by interfixation movements in reading is relatively small and varies with kind of material read. Tinker (109) found that the per cent of reading time devoted to moves ranged from 5.3 to 9.6 with an average of 7.3. The more careful and analytical the reading, the smaller the relative time taken by moves. Luckiesh and Moss (63), after noting that there is no clear vision during horizontal eye movements in reading, state, "However, this unique and advantageous arrangement is stated to be absent in the case of vertical movements of the eyes." There is no sound basis for the latter statement.

Monocular and binocular vision. Apparently oculomotor behavior is about the same whether one or both eyes are used in reading. Knehr (51), employing eye-movement measures, found no significant differences in performance between monocular and binocular reading. Furthermore, the oculomotor patterns were alike under the two conditions. Clark (23) found that alternating vision which occurs during bar-reading, did not appreciably alter reading efficiency as measured by eye-movement records. Further experimenting with a larger group of subjects and with a more adequate sample of reading is needed to check these results. Nevertheless there is evidence to support the above findings. A mass of data indicates that central factors of comprehension and assimilation rather than peripheral, i.e., mechanical factors, are dominant in determining reading proficiency.

Factors in Variability

Variation in subject matter. For some time it has been recognized that variation in subject matter produces changes in eye movements.

Additional information on the nature of the variation is contained in the following studies. Seibert (89) made an analytical study of eye movements of 8th grade pupils while reading mathematics, biography, adventure, physical science, history and geography. Differences in all eye-movement measures occurred in going from one subject matter to another, but these differences are not as significant as the author appears to believe. The critical ratios ranged from .00 to 2.53. It is admitted that some children maintained a definite pattern of eye movements throughout all materials. This indicates a lack of flexibility in reading habits. A similar analytical investigation was completed by Stone (98) with college freshmen as subjects. They read arithmetic, biology, English, educational psychology, physical science and social science. Although the critical ratios are not large, differences that tended toward significance were obtained for eye-movement measures in going from one kind of material to another. Implications of the findings were not discussed. Both of these studies may be criticised for faulty statistical interpretations.

Condition of reader. This category is concerned with such factors as maturity level, reader proficiency, stuttering and deafness. Buswell (14) has investigated the eye movements of adult readers. Measures employed for diagnosing reading maturity include:

1. The span of recognition or amount of print recognized in a single pause depends directly upon fixation frequency. A relatively wide span indicates greater maturity.
2. Rhythmic regularity of progress of eye movements along the line of print is considered symptomatic of a high degree of maturity in reading.
3. For any given kind of reading the more mature reader will make fewer regressive movements than the immature reader.
4. The more mature reader employs eye-movement pauses of briefer duration.

On all these measures the adult readers tended to be less mature than high school seniors. In fact the adults were only slightly better than 6th grade children in reading material within the comprehension range of both children and adults. Buswell emphasizes that reading habits should be flexible, i.e., they should adjust themselves to the kind of material read. Nevertheless, he failed to note that in general his adult readers revealed a marked lack of flexibility of oculomotor behavior in going from easy to more difficult material.

Anderson (2) compared the oculomotor patterns of 50 good and 50 poor readers to find how such readers adjust to changes in the difficulty of the reading material, and to reading for different purposes. All eye-movement measures distinguished good from poor readers. For both

groups, fixation frequency, regression frequency and pause duration increased with the more difficult reading material. Good readers showed a greater flexibility in eye movements in adjusting to increasingly difficult material. Greatest irregularity in oculomotor patterns occurred in reading to remember details. Here the differences between groups was present but less pronounced. It was concluded that two important determinants of regularity in eye movements are difficulty of material read and purpose of reading. In a qualitative analysis of eye movements of good readers, Walker (120) discovered that on the average fixations were distributed evenly throughout the line for both easy and difficult material. Pause duration was greater in the first part of lines. About 80 per cent of lines were covered by fixations and the last fixation was nearer the end of the line than the first. Extent of forward shift of eyes was not related to duration of fixation at end of the shift.

There is not complete agreement concerning the oculomotor patterns of stutterers. While Moser (73) found that eye movements in silent reading are less effective for stutterers than for normal children, Hamilton (42) found no differences. The former considers that stuttering may cause reading disability while the latter states that stutterers do not need special treatment in the silent reading program. Since Hamilton's experiment was more adequately controlled, the more acceptable view is that eye movements in silent reading are the same for both stutterers and normal readers. In Moser's (73) study, silent fixation was the same for stutterers and normal speakers, but when stutterers talked during fixation, a variety of eye movements occurred. During oral reading, differences in oculomotor patterns were associated with stuttering spasms. These findings suggest that extrinsic eye muscles as well as other muscle groups are not under control during stuttering. Working with another kind of handicapped group, La Grone (56) found that the eye-movement patterns of deaf children differed from those of hearing children. This was especially true for developmental levels in reading. Although pause duration tended to decrease from grade to grade, fixation and regression frequency increased and then decreased. The most common kind of regression occurred at the beginning of lines, but there were fewer regressions for deaf than for hearing children. Since the groups studied were very small, these findings need checking.

Oral versus silent reading. A few studies have been concerned with eye movements in oral reading. Buswell (14) points out that schools have recognized that the processes of oral reading and of silent reading are psychologically different. Experimental work yields evidence on the

degree of this difference. The eye movements of good, poor and unselected college subjects during silent reading were compared with eye movements during oral reading by Anderson and Swanson (4). The correlations for the eye-movement measures ranged from .50 to .70. Coefficients for the poor readers were somewhat higher. In silent reading there were fewer fixations and regressions and shorter pause durations. Going from poor to good readers, the differences in eye movements between oral and silent reading increased. Thus the eye-movement patterns tended to be more similar in the two kinds of reading for poor readers. These data show, therefore, that there are common elements in oral and silent reading, especially among poor readers. According to Buswell (14) silent reading of adults differs from oral reading in respect to the degree that vocalization is suppressed. Evidence from eye-movement measures while silently reading material designed to exaggerate vocalization supports this view. Under these conditions the eye movements of poor readers were like movements in oral reading; those of good readers were not.

In studying the eye movements of adults during oral reading, Buswell (14) found that the mean eye-voice span is an excellent index of the maturity of a person's oral reading habits. The greater the span the more mature the reading. An excellent study of eye movements of good and poor readers during oral reading is reported by Fairbanks (31). Both eye and voice records were made. As in the above study the eye-voice span reflected maturity of reading habits. Although difficulty of textual material determined variations in eye-movement measures, the good readers were less affected than the poor readers. No fixed pattern of eye movements was discovered. Analysis of regressions in relation to eye-voice span together with a comparison of the eye-movement patterns of good and poor readers led to the conclusion that faulty eye movements do not cause errors in reading but that the errors are central in origin.

Miscellaneous. Certain studies are not easily classified under the above groupings. Simpson (92) found no significant correlation between eye-movement measures and time spent in leisure reading. It is concluded without justification that eye movements are not improved unless specifically trained. In another study, Simpson (91) obtained correlations of $-.38$ to $-.52$ between fixation and regression frequency versus college achievement and mental ability. Pause duration showed no appreciable correlation with achievement and intelligence. Reading test scores correlated higher than eye-movement measures with achieve-

ment and intelligence. The inference that eye-movement training is desirable because the correlations are low is a misinterpretation of the data (see *training eye movements* p. 110). Imus, Rothney, and Bear (45) found that the correlations for eye-movement measures versus college aptitude, scholastic achievement and reading test scores were only .07 to .38. Their conclusion that eye-movement measures are unreliable and invalid is not justified (see *reliability* p. 94 and *validity* p. 95). In a similar study with children, Leavell and Sterling (58) found low to moderate correlations between eye-movement measures and intelligence. Taylor (105) obtained significant differences between eye-movement measures of children making normal school progress and of those failing. The grade norms given by Taylor are of doubtful value because an unreliable sample of eye movements was obtained. In fact Taylor was largely responsible for providing the inadequate samples of reading material employed by subsequent users of the Ophthalm-o-graph. They are too short to yield reliable records.

Peripheral vision. For years writers have been emphasizing the importance of peripheral vision in reading. Cues perceived in peripheral vision not only furnish premonitions of coming words and phrases, but they also guide the movements of the eyes to new fixations along the lines of print. La Grone (55) has studied the relationship between peripheral vision and eye movements in reading. Accuracy of perception in the left and in the right peripheral fields was obtained from tachistoscopic exposure. High perception scores in the left peripheral field tended to go with fast reading, few fixations and shorter pause duration. But high perception scores in the right peripheral field tended to go with slow reading, more fixations and longer pause duration. Regression frequency was not significantly related to perception scores. Higher perception scores either in right or left peripheral fields are said to be due to preference. Since vision in the right peripheral field is supposed to be important in reading, why do the more able readers have better perception to the left? The author misses the point that in tachistoscopically exposed material the long practiced habit of reading material from left to right is dominant. It is logical to infer that such a habit is more effective for good than for poor readers. Furthermore, a short exposure test of peripheral vision is not an adequate measure of peripheral vision as it operates in the normal reading situation where the successive fields of vision overlap. While the findings in this study are interesting, they do not provide a crucial test of the role played by peripheral vision in normal reading.

Special Reading Situations

Spelling. In a follow-up of an earlier study on spelling (see 107), Gilbert (33) obtained records for six children over a three-year period. The eye movements revealed growth characteristics in perceptual habits. Although there was little change in pause duration, fixation and regression frequency decreased and there was evidence of a more effective method of attack in studying the words. The more mature spellers differentiated between hard and easy sections of words and their ocular responses tended toward simple verification reactions. Gilbert and Gilbert (35) recorded eye movements before and after training for speed and accuracy of perception in learning to spell with 4th, 5th, and 6th grade pupils. Training resulted in less perception time per word and fewer fixations and regressions, but no change in pause duration. Thus the training resulted in a larger fixation span and less attention to details, but the general pattern of cross study and accuracy of spelling was maintained. Here is a good example of how training in perception habits with no attention to eye movements yields changed oculomotor patterns which reflect more effective central processes of apprehension and assimilation. In a later study, Gilbert and Gilbert (36) found that spelling is learned more effectively when words are studied in isolation than when encountered as critical words in reading. Eye movements revealed that when words were in text the reading was disrupted, especially for poor spellers. The authors' suggestion that a concentration of fixations may represent spelling reactions rather than comprehension or recognition difficulties would hold only if the readers were set to learn spelling as they were in this experiment due to a prior spelling test.

Reading music. The reading of music is a highly specialized oculomotor task which involves both vertical and horizontal eye movements. Lowery (60) notes that individual differences are in terms of pause duration rather than fixation frequency. Weaver (124) obtained photographic records of eye movements and of keyboard performance for 15 trained musicians during sight reading and playing of three selections at a piano. From one to two notes were read per fixation. A musical note is considered equal to a word in reading. Pause durations are longer in music reading than in word reading and vary with the complexity of note relations. The author's correlations of .82 to .89 between pause duration and reading time and of only .27 to .58 between fixation frequency and reading time are in contrast to word reading where fixation frequency yields the high correlations. This supports Lowery's (60) finding that pause durations provide the individual variation. According to Weaver (124), predominance of horizontal or of

vertical progression of eye movements depends upon the nature of the music. Notes on the treble staff are usually but not always read ahead of notes on the bass staff. The eye-hand span was highly variable. Regressions appeared to be related to kind of music read. In another study, Van Nuys and Weaver (119) investigated eye movements of 12 experienced musicians while reading musical rhythms and melodies of 12-note phrases, each presented for a period of 2.8 seconds. Memory span equalled the number of notes correctly played for each phrase exposed. Regressions occurred more for the melodies than for rhythms while pause duration increased with increase in complexity of note relations for both melodies and rhythms. Pause duration tended to be longer for rhythms than for melodies and there were more notes correctly played per fixation for the rhythms. Findings in these studies of eye movements in reading music furnish material for a comparison with the reading of words:

1. Considering a note comparable to a word, the fixation span is similar in both kinds of reading.
2. Regressions serve somewhat the same purpose in both situations.
3. Increase in complexity of text brings increase in complexity of eye-movement patterns for both music and words.
4. Reading music involves both vertical and horizontal eye movements while reading words in text involves only the latter. There are of course minor vertical deviations of the eye in reading a line of print. And the return sweep to the next line involving both vertical and horizontal displacement, is common to both situations.
5. In reading words, fixation frequency is the best indicator of reading rate but in music pause duration is best.

Hereditary and Certain Environmental Factors

Twin studies. Two studies of twin similarities in eye-movement patterns while reading prose have been reported. Morgan (72), comparing eye-movement measures for members of pairs in a well controlled experiment, found very low correlations (.04 to .24) between artificial pairs, higher (.24 to .53) for fraternal twins, and still higher (.61 to .72) for identical twins. These coefficients indicate more fundamental bases for individual differences in eye movements in reading than is furnished by a "habit" concept. The "underlying processes of assimilation" constitute a more logical explanation of the results. Hereditary factors play a significant role to the extent shown by the correlations. It is noteworthy that pause duration, which is least susceptible to training, showed the highest correlations for twins. The author correctly infers that, in treating poor reading, consideration should be given to capabil-

ities of the individual rather than exclusively to certain ideal motor sequencies characteristic of oculomotor patterns of efficient readers. In a second study, Jones and Morgan (50) report additional evidence on twin similarities in eye-movement patterns. Median correlations for eye-movement measures in reading revealed a trend similar to that in the earlier study: artificial pairs, r = .105; fraternal twins, r = .435; and identical twins, r = .530. Three judges had considerable success in identifying on the basis of general appearance the eye-movement photographs of identical twin pairs when they were mixed with photographs from fraternal twins. Considering the many factors which may modify oculomotor patterns, it is significant that this identification was better than chance. These results, therefore, support the earlier conclusion that eye-movement patterns in reading are not determined wholly by training. Genetic factors are involved to an appreciable degree.

Emotion. Emotional disturbances appear to modify eye-movement patterns. Strongin, Bull, and Korchin (102) report that when readers expect a shock, visual efficiency in terms of eye movements decreased by about 10 per cent and binocular coordination became worse in 14 per cent of readers. Warren and Jones (122) obtained eye-movement records while subjects read in the laboratory and while they were exposed in a high place. No significant differences in fixation and regression frequency or pause duration were found. But unsteadiness of fixations appeared when in the high place, especially for acrophobic subjects.

Illumination. Luckiesh (61) and Luckiesh and Moss (64, 65) report the effect of variation in illumination on eye movements in reading. Records were obtained at the beginning and at the end of one hour of reading under one foot-candle and again under 100 foot-candles of light. Speed of reading was slowed up significantly greater under the one foot-candle of illumination in comparison with the 100 foot-candles. The difference was due largely to changes in pause duration. Although the authors condemn speed of reading as a measure of readability of print, they employ eye movements for this purpose, apparently unaware that eye-movement measures are speed of reading measures.

Readability. Eye movements have been used extensively by others to determine legibility or readability of print. Bell (8) found no difference in eye movements for reading typewriting versus manuscript writing, but a significant difference in favor of typewriting versus cursive, and in favor of manuscript versus cursive writing. Differences were mainly due to changes in fixation frequency rather than pause duration. Tinker and Paterson have completed eight eye-movement studies on the readability of type. In a long series of studies (78) these

authors had compared speed of reading for various typographical variations. Significant differences were found in certain comparisons. The analytical eye-movement studies were undertaken to discover the specific patterns of eye movements related to the disclosed differences. The non-optimal text, which was read slower than the more readable material, was characterized by the following changes in eye-movement patterns:

1. All capitals (slower) versus lower case—more fixations (113).
2. Old English (slower) versus a modern type face—more fixations (114).
3. Ten point type: 19 versus nine pica (slower) line widths—more fixations and longer pause duration; 19 versus 43 pica (slower) line widths—more fixations and regressions plus longer pause duration (79).
4. Six point type: 13 versus five pica (slower) line widths—more fixations, fewer regressions and longer pause duration; 13 versus 36 pica (slower) line widths—more regressions and longer pause duration (81).
5. Type size: 10 versus six point (slower)—more fixations and longer pause duration; 10 versus 14 point (slower)—more fixations and longer pause duration (80).
6. Type sizes in optimal line widths: 11 versus eight point (slower)—more fixations and longer pause duration; 11 versus six point (slower)—longer pause duration (82).
7. Black print on white background versus red print on dark green background (slower)—more fixations and regressions plus longer pause duration (115).
8. Several variations: optimal versus non-optimal (slower) typography—more fixations and regressions plus longer pause duration (83).

These analytical studies of eye movements in relation to typographical variations indicate that non-optimal conditions may only increase fixations or pause duration, or both of these. Regressions may increase or decrease. Factors operating to change the oculomotor patterns include changes in visibility (small print, low contrast between print and background), in word form (large print, all capitals), prevention of optimal use of peripheral vision (very long and very short lines), or lack of familiarity with a type face (Old English).

Visual fatigue. Eye movements, along with other measures, have been employed in the search for adequate measures of visual fatigue. Dearborn (26) presents some tentative data which suggests that eye movements may reveal visual fatigue. After a long day of visual work consisting of proofreading and study, speed of reading was reduced and saccadic movements through relatively long angles were retarded. Since only two readers served as subjects, these trends need checking. According to Clark and Warren (25), during a 65-hour vigil there was no consistent trend in variation of fixations, regressions or pause duration

for eye movements in reading. Although sporadic and temporary changes in binocular behavior occurred for certain individuals, there were no uniform trends as the period of wakefulness increased. Since compensation is possible, eye movements apparently failed to furnish an adequate picture of fatigue effects. Kurtz (54), however, claims that eye-movement records reveal fatigue after 30 minutes of severe visual exertion. It is difficult to evaluate his data as presented. The most thorough evaluation of eye movements in reading as measures of visual fatigue has been made by McFarland, Holway and Hurvich (69). Thirty minutes of either rapid saccadic exercise or 30 minutes of rapid convergence-divergence movements failed to produce any significant changes in eye movements while reading. Although their results were negative, the authors consider that eye movements ought to provide a sensitive indicator of visual fatigue if a more sensitive recorder were available. Such an apparatus would yield a more precise measure of fixation and of binocular coordination. But fixation and regression frequency and pause duration in reading would remain unchanged.

In general these studies reveal that eye movements in reading are insensitive indicators of visual fatigue. Only when the fatiguing task is extremely severe is there a suggestion that eye movements are modified. Two factors are probably preventing fatigue from affecting eye movements in reading: (a) All experimenters have used only 10 to 12 lines of text to be read before the camera. The visual mechanism can compensate enough to give normal performance for the short periods of time required to read such materials. (b) Furthermore, there is no adequate way to evaluate precisely the degree of comprehension in the short paragraphs of text ordinarily used. The reader, since there is a tendency to maintain one's oculomotor pattern, may read two paragraphs with similar eye movements but with different degrees of comprehension. An adequate check of eye movements as measures of fatigue can be obtained only with long samples of reading text equated for difficulty and so constructed that comprehension is satisfactorily measured or maintained constant.

Anoxemia. Definite and consistent modification of eye movements in reading occurs with anoxemia. McFarland, Knehr, and Berens (67, 68) found significant increases in reading time, i.e., perception time plus eye-movement time, and fixation frequency in oxygen concentrations corresponding to 15,000 and 18,000 feet. Of greater importance, perhaps, was the qualitative variation of ocular movements in oxygen want. That is, nystagmoid tendencies and general unsteadiness appeared and there was an accentuation of abnormalities such as muscle imbalance.

Furthermore, latent oculomotor defects frequently became apparent indicating that the procedure may have clinical significance. In general, altered oculomotor behavior yielded a sensitive measure of the early effects of anoxemia.

Eye Movements in the Reading Clinic

Views concerning the usefulness of eye-movement records in the reading clinic vary. Kurtz (53) is probably over optimistic when he claims that the importance of a permanent objective record, i.e., eye movements, can not be over-estimated. The view of Clark (21) that eye-movement photography serves as a valuable objective auxiliary to other techniques is more moderate. Opposed to these is the view of Imus, Rothney and Bear (44, 45) who, in appraising eye movement photography as a clinical method, are convinced that the technique lacks both reliability and validity. (See criticism of this view under *reliability* p. 94 and *validity* p. 95.) Tinker (110), in an evaluation of eye-movement measures, notes purposes for which they may be used. He concludes, however, that eye-movement records are not indispensable in clinical diagnosis of reading status. An exaggerated stress is placed upon the study of eye movements in the clinical situation by Hamilton (41) and Sievers and Brown (90).

Orthoptic training. Peters (84) found that training with the Binocular Synchronizer and the Squint Korrector yielded a reduction in fixation and regression frequency. These changes, however, were similar to those produced by motivated reading practice alone. More extensive studies of this kind are reported by Parkins (75, 76, 77). Apparently there was marked improvement in reading with corresponding changes in eye movements due to orthoptic training. In this group of studies, the results are so presented that it is difficult to evaluate them. The reviewer is inclined to reserve judgment until check studies are made.

Visual abnormalities. Extensive clinical use of eye-movement records has been made by Clark. In an initial study (17), comparing the eye movements of individuals having normal binocular balance with those having muscular imbalance (exophoria), revealed no differences in either fixation frequency, regression frequency, pause duration, or time required to complete divergence movements at the beginning of the lines of print. For similar comparisons in later reports (18, 19, 22), Clark also found no difference in fixation and regression frequency but there were differences in vergence movements during reading. The exophoric readers made significantly greater divergence movements at the beginning of lines and required more time to complete these corrective divergence

movements. It is suggested that the large divergence movements of subjects with muscular imbalance may cause enough fatigue to be important in remedial reading programs. Clark (20) reports similar trends for a case of high exophoria. After noting that his experiments reveal no striking differences in oculomotor patterns between normal readers and those with muscular imbalance, Clark (24) concludes correctly that such deficiencies are apt to cause undue fatigue and should be corrected. His belief, however, that visual deficiencies cause reading disability in spite of the negative evidence is not well taken. In a correlational study, Stromberg (100) found no relation between amount of vergence movements and phoria of eye muscles. This, however, does not negate Clark's contention that it is desirable to correct muscle imbalance for the individual case.

Remedial reading. In an extensive study of remedial reading at the college and adult levels, Buswell (15) employed the eye-movement camera to measure with precision the changes in perceptual habits which occurred during the experiment. He states, however, that an eye-movement camera should be considered a research rather than a clinical instrument. Simpler methods of measurement which are entirely satisfactory in an ordinary remedial program are available. Once a procedure in the reading clinic has been validated by eye-movement records, there is no need to recheck the procedure in other remedial groups.

Training Eye Movements

Problem. The early experimenters were concerned with identifying and describing oculomotor behavior during reading. As this field of investigation developed, differentiating factors in eye-movement patterns were more emphasized. That is, stress was placed upon developmental variation, differences between good and poor readers, variation with changes in subject matter read, etc. This eventually led to the practice of attempting to improve reading performance by training eye movements. The necessary assumption is that eye movements are important determinants of reading proficiency. Thus, if the retarded reader is trained to use eye-movement patterns similar to those which characterize efficient reading, his reading proficiency would presumably improve. This emphasis upon the mechanics of eye movements tended to direct attention to peripheral factors as determinants of reading performance rather than to the central or underlying processes or perception, apprehension and assimilation.

Methods. Techniques of training eye movements in reading vary. On the one hand are those, represented by Pressey (see Tinker, 108) and Sievers and Brown (90), who try to train subjects to pace eye fixations at three regular intervals along a line of print. It is true, however, that Sievers and Brown suggest that eye-movement training is but one small part of a reading program. Nevertheless it is naïve to assume that if a line of print is separated into three groups of words, that paced eye movements will result in reading each group with one fixation. In fact, Dearborn and Anderson (27) have demonstrated that such an arrangement results in more than three fixations. Buswell (15) also notes that few subjects ever achieve three fixations per line. At the other extreme are those who attempt to increase the fixation span by employing techniques which approximate the normal reading situation. Thus, Dearborn and Anderson (27) have devised a flexible film projection method for teaching phrasing to increase the size of fixation span. Approximately the same principle is involved in the still-film projector method developed by Buswell (15) to increase span of recognition or fixation span. Successive phrases of text are exposed in proper sequence and spacing to lead the eye along in normal perceptual sequences. Timing and nature of textual material are flexible in both techniques. Between these extremes a variety of techniques have been employed to train eye movements. The most commonly used one is the metron-o-scope designed to develop "controlled reading." It is described by Taylor (105). This is a mechanically operated machine for exposing each line of print in three successive sections. Taylor (105) states that the fundamental idea of the metron-o-scope is to develop the mechanical, i.e. eye movement, and interpretative processes simultaneously. As noted above, such techniques do not reduce fixations to three per line.

Controlled reading. A large number of reports describe training of eye movements or controlled reading. Since Anderson (1, 3) Sisson (95), Kottmeyer (52), Nelson (74), Pilant (85) and Traxler (117) have evaluated or reviewed much of this material, detailed notice to the separate studies will not be given here. The experimental reports on controlled reading include studies by Broom (10, 11), Dearborn and Anderson (27), Dodson (28) Garver and Matthews (32), Hallock (39), Lauer, Henry and Fritz (57), Lee (59), Robinson (86), Simpson (93), Taylor (105), and Witzeman (125). Although some of these studies were done without adequate controls and lacked acceptable statistical treatment, they are rather uniform in showing considerable improvement in reading due to the training. Three studies in this area will be surveyed in more detail.

Sisson, (96), using matched groups of adult readers found that reading with intent to improve produced significant changes in eye movements comparable to the changes from eye-movement pacing. In a more elaborate study, Cason (16) employing matched groups of school children, found no significant differences in improvement in reading for the students trained in phrase reading or in eye movements by use of the metron-o-scope in comparison with those who did motivated library reading. That is, free library reading resulted in the same gains as programs stressing the oculomotor mechanics of reading. All made significant gains. Buswell (15) employed a film projector for controlled practice to increase the fixation span or span of recognition. This resulted in improved reading proficiency which was reflected in changed oculomotor patterns. Emphasis was upon apprehension of broader perceptual units so that the reader would be freed from the requirements of detailed visual perception. This permitted greater attention to meanings.

Evaluation. Eye-movement training may be evaluated as follows:

1. Many so-called procedures for training eye movements or for controlled reading result in improved reading efficiency.
2. The improved reading status is reflected in modified oculomotor patterns.
3. The improvement obtained by eye-movement training, with or without elaborate apparatus, is no greater than that resulting from motivated reading alone.
4. There is no adequate evidence that training eye movements as such improve reading (107). Experiments concerned with pacing eye movements and controlled reading usually involve other techniques and are never divorced from increased motivation. Buswell (15) flatly states "training eye movements does not increase reading ability."
5. The training of eye movements too often becomes a ritual and tends toward an overemphasis upon oculomotor mechanics to the sacrifice of adequate attention to the more important processes of perception, apprehension and assimilation. This training may result in a decrease in the flexibility and adaptability of reading habits which characterize good readers. According to Buswell (15), "The exploiting of machines and gadgets" to control reading "by persons who do not understand the psychology of reading seems at present to be adding greatly to this mechanistic folly." And Traxler (117) has "difficulty in seeing any justification for purely mechanical attempts to train pupils in better eye movement habits."
6. The reviewer agrees with Traxler (117) that controlled reading techniques, i.e., training eye movements, "need not be used in a purely mechanical way but that they might well be used to supplement, in a way that would have strong motivating force," a program in which reading comprehension is emphasized. The reviewer believes, however, that as long as gadgets are used by those with an inadequate understanding of the psychology of reading, we will continue to have the undesirable emphasis upon oculomotor mechanics.

Central Versus Peripheral Processes

Problem. Training of eye movements is intimately related to the problem of whether eye movements are causes or symptoms of reading proficiency. The view that eye-movements are causes of reading proficiency is based upon the discovery that when there is ineffective reading, there are many fixations and regressions and that good readers use few fixations and relatively few regressions. This trend developed until the point was reached where it was claimed that the good reader is good because he uses few fixations and rarely makes a regression. Thus, to develop a good reader presumably it is only necessary to develop effective oculomotor habits. This gradually led to a strong emphasis upon peripheral factors, especially among the large group of writers with an inadequate knowledge of the psychology of reading. A few writers, including this reviewer (107, 108, 111) began to question this emphasis. It was pointed out that the more important determinants of reading proficiency were the central processes of perception, apprehension and assimilation. These processes are reflected in oculomotor patterns because eye-movement habits are very flexible and appear to adjust themselves readily to any change in the perceptual processes involved in reading. It would appear that eye movements merely reflect efficient or poor reading performance rather than cause it.

Recent evidence and conclusions. There is really no evidence to support the view that eye movements determine reading proficiency. As shown in the above analysis, the results obtained from training eye movements do not support the view that peripheral factors are dominant although based upon the assumption that they are. On the other hand, there are numerous reports which indicate that the central processes are the important determinants in reading performance. Analysis in which the writers conclude that eye movements reflect the central processes of perception, apprehension and assimilation include: Anderson (1, 3), Traxler (117), and Sisson (95, 97). A variety of recent experimental studies of eye movements have led to the conclusion that oculomotor patterns reflect poor and efficient reading, not cause it. These include: Anderson (2), Fairbanks (31), Knehr (51), Rogers (88), Sisson (94), Swanson and Tiffin (104), and Walker (120), and Walker and Molish (121). For earlier studies see Tinker (107, 108). Changes in eye-movement patterns occur when difficulties of comprehension arise, with variation in reading purpose or attitude etc. It is now well established that oculomotor reactions are exceedingly flexible and quickly reflect any variation in the central processes of perception, apprehension and assimilation.

Evaluation. Careful consideration of the field would probably indicate that there need be no conflict in assigning proper roles to peripheral and central factors in reading. To read, one must perceive words in proper sequence and to do this the eye fixations must progress in appropriate patterns. In other words, oculomotor patterns are a means of achieving the perceptual sequences necessary for apprehension and assimilation of the textual material. The particular eye-movement pattern employed is conditioned by the nature of the central processes. Clear apprehension and rapid assimilation of the textual material is reflected in relatively few fixations and regressions. But ineffective perception and confused apprehension, accompanied by difficulties in assimilation, characteristically produce many fixations and regressions. The latter, however, should not be confused with the oculomotor pattern occurring in highly analytical reading where it is necessary to reexamine the text and to work out relationships. In such a case, as shown by Tinker, Frandsen and others (see Tinker, 107), the complex eye-movement pattern may indicate effective reading rather than inefficient central processes. It seems possible, therefore, that the unfortunate dichotomy of central versus peripheral processes might be avoided to some degree by omitting use of the terms eye-movement patterns and rhythmical eye movements, and by employing instead the concept of perceptual sequences. The latter is more meaningful in discussing the reading process and it avoids the unfortunate mechanical implications of the former.

Summary Statement

Important progress in the study of eye movements in reading has been made during the past ten years. Many of the investigations have been of high quality. Nevertheless unfortunate trends have appeared. Many unqualified individuals, those without an adequate background in either experimental procedures or the psychology of reading have been doing "research" in eye movements. The results are deplorable. Furthermore, the exploitation of the eye-movement technique has accentuated the unfortunate emphasis upon peripheral determinants of reading proficiency and has led to wide use of eye-movement measurement in the clinical situation although the eye-movement camera should be considered a research rather than a clinical instrument. There are signs, however, that the conclusions from sound research and the views derived from adequate analysis of results in the field are beginning to make headway.

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COLOR ADAPTATION TO 1945*

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It is the purpose of this paper to present a critical history of the phenomenon known to workers in color as color adaptation, color fatigue, or color minuthesis.

Although usually attributed to later investigations, the first scientific observation of color adaptation appears to be that described by the French mathematician and physicist De La Hire (12, p. 289) in 1694. His was a description of observations of the sun, the course of perceptual change, and the resulting after image. He ventured an hypothesis concerning the fatigue of visual receptors, and although it is somewhat erroneous in view of later investigations, it is characteristically the Hering theory.

The first important investigation was the 1863 contribution of Maria Bokowa (5). Bokowa wore copper oxide glasses of 1 mm. thickness from four to five hours. The glasses had a transmission which was predominantly red, but they could not have been highly selective since she was able to see colors in all parts of the spectrum. The experiment was controlled, in a manner, since a spectrum was viewed from time to time during the course of adaptation and comparisons made from one observation to another. Further, there was an attempt at quantification by viewing a color wheel and describing the perceptual color which corresponded to a physical stimulus resulting from an arbitrary number of circular degrees of arbitrary "primaries." In general, Bokowa's results may be summarized as follows:

1. After color adaptation had taken place, all colors appeared as either yellow or blue.
2. The brightest color was a mixture of yellow and blue.
3. The cut off point of the visible spectrum shifted toward the short end of the spectrum.

It is to be noted that nowhere does Bokowa indicate that the adaptation phenomena continued to a neutral gray although Helson and Judd (19, p. 383) refer to Bokowa as having reached complete adaptation. Bokowa (5, p. 163) refers to red as appearing less intensive during adaptation and this indicates that some change in intensity may have taken place. She may, however, have been referring to a loss in satura-

* From a doctoral dissertation *The Color Adaptation of the Human Eye* directed by Professor H. P. Weld. For an additional paper based on the same dissertation, see Cohen (11).

tion although the word saturation does not appear in the paper. Bokowa, then, reports changes in hue, and makes references to changes in intensity.

In 1865, the phenomena was noted by Aubert (3) in his observations of red and blue colored squares against achromatic backgrounds. The loss of saturation was referred to as (3, p. 131) "Adaptation der Netzhaut für Farben." He adds that adaptation for blue light is faster than adaptation for red light. It is interesting to note that Sheppard (33, p. 35) says, "Nowhere in his writings does Aubert connect this term [adaptation] with color-changes, for which he uses the term 'fatigue'." The use of the term adaptation for both white and colored light apparently did originate with Aubert.

Sigmund Exner's experiment (14) in 1868 was the first which used a spectrum as adapting and reacting stimuli. His experimental procedure was to throw a spectrum on a screen. Slits were placed in the screen so that the light of the adapting color came through one slit while the light of the reacting color came through the other. Exner was, of course, studying the effect of the after image as well. By observing at a distance the beam of light which penetrated the first slit, he adapted a small area of the retina. The adaptation time was ten seconds. The eye was then brought very close to the second slit which had the effect of stimulating a much larger area of the retina. And in this larger area there appeared the after image of the light stimulus of the first slit. Inspection of Exner's table shows the following results:

1. When the stimulus color and the adapting color are identical, the adapted portion of the retina is always lower in saturation than the non-adapted portion.
2. The adapted portion of the retina perceived all colors as red, green, or blue.

With respect to this second result, Exner attempted to locate the spectral position of these basic colors whose hue does not shift during adaptation and toward which all other hues shift. For this purpose he devised an instrument utilizing a telescope and a spectrum. His results are as follows:

1. For red the position is from the long end of the visible spectrum to a point between C (656 mμ) and D (589 mμ), perhaps somewhat closer to C.
2. For green the location is between E (527 mμ) and b (517 mμ).
3. For blue it is in the immediate environment of the line G (431 mμ).

One of the few early studies directly concerned with the changes in intensity during color adaptation was performed by Schön (31) in 1874. Schön was not interested in adaptation phenomena as such but rather in the physiological mechanism to which discrepancies among previous

experimenters who had determined the differential sensitivity of the retina to the different portions of the spectrum could be attributed. Schön suggested that the differences between the many investigators might be due to the color adaptation of the retina.

Schön used prisms to obtain spectral stimuli. These stimuli were presented to an observer through a tube which was divided in half forming a stimulus and comparison field. The intensity of each half was controlled by a variable slit, the width of the slit being a measure of the intensity. The experiment was begun by making the slits equal on both halves of the tube. The comparison field was shielded from the view of the monocular observer while the stimulus was presented for 3, 5, 10, or 15 seconds. At the end of the interval the comparison field was presented to the subject who stated whether they were equal in intensity. If they were not judged to be equal, the slit of the comparison field was changed and the determinations continued until they were judged to be equal. Three stimulus colors were used—red, green, and blue. Schön came to these conclusions:

1. After five seconds exposure to spectral red, green or blue, one half of the brightness is lost.
2. There is a further decrease in brightness which takes place gradually.
3. This further decrease is greatest for blue and least for red.

Many criticisms have been leveled at this experimental procedure and consequently the results. His two test fields were not strictly comparable because of the construction of the apparatus. He ignored the differential sensitivity of the retina to various wave lengths in comparisons of heterogeneous wave lengths. For other criticisms and an alternative procedure which avoids most of the defects, see Almack (2, p. 27 ff).

Ssamujlow's 1889 experiment is only reported in abstract form (34). So meager is the information available that it is difficult to determine the exact nature of his investigation. Ssamujlow considered the duration of the after image as a measure of the adaptation of the retina, a theory which may not be strictly true. Ssamujlow does not adequately explain what he means by adaptation, but presumably he refers to loss in saturation. Using spectral light sources, he arrived at the following results:

1. The retina does not adapt equal amounts for different lights. It is most for red, less for green, and still less for blue.
2. The degree of the adaptation is dependent on the time and the strength of the stimulus.
3. The peripheral retina adapts to a lesser degree than the fovea.

There are two contributions to the literature by Carl Hess. The first (20), published in 1890, describes his experimental procedure. A telescope was arranged in conjunction with spectroscopes so that there appeared in the telescope a left and right field, either of which could be illuminated with monochromatic light. The field on the left was a square while the field on the right had the form of a circle. The telescope could be shifted so that either the right or left field came into view. The observer's eye was placed at the telescope and fixated the right edge of the left square field. After a given interval the telescope was shifted to the right field with the cross hairs in the center of the circle. Thus only one half of the adapted retina is covered with the new fixation, so that changes which occurred by virtue of the adaptation process are apparent by comparison of the fields. Nine different homogeneous lights and two non-spectral lights made by mixing spectral red and spectral violet were used as both adaptation and reaction stimuli. Changes in hue were noted for all combinations of lights taken two at a time; each light was used as an adaptation light and matched with all other lights as reaction stimuli. There were two adaptation times—10 seconds and 35 seconds.

The experiment was continued in 1893 (21) and here the apparatus was modified to give results in terms of wave length. The spectroscope was fitted with two adjustable slits; through one slit passed the adapting light while the reacting light passed through the other. The other spectroscope was fitted with a single adjustable slit. The telescope was not kept in a fixed position. The left half of the telescopic field could be filled with either the reacting light or the stimulus light while the right field was filled with light from the adjustable single slit of the right spectroscope. The observer fixated a point between the two fields for 50, 70, 75, or 180 seconds. The left field was illuminated with the stimulus light while the right field showed blackness. At end of the adaptation time the stimulus light was shut off and the reacting light and the comparison light presented simultaneously. The comparison light was readjusted after successive trials until it matched the perception of the left field. Hess reaches these conclusions:

1. There is a tendency for colors to appear as either blue, green, or red to the color adapted eye. This is only a tendency and the data present an erratic appearance. Hess' cursory attempt to locate the wave lengths of these colors using only three colors as stimuli (having theoretical implications in the Helmholtz theory) found them to be 490 mμ for blue, 565 mμ for green, and 650 mμ for red. These figures are quoted erroneously by Almack (2, p. 26) and Sheppard (34, p. 35 ff). To be sure, the basic colors are not well identified, but the data do indicate a red basic.

2. The shift in hue increases with time. There is no reference to saturation in this paper.

Voeste's 1898 experiment (45) is one of the keystones in the history of color adaptation. His apparatus was the Helmholtz color mixer (18, p. 395 ff) as described by König and Dieterici (22). Essentially two fields were presented to the subject. The observer monocularly fixated a point between the left stimulus field and the right comparison field. The stimulus field was illuminated with a monochromatic source while the comparison field was unilluminated. After ten seconds, the observer matched the right field with what was seen on the left. It was matched for hue by variation of the prismatic surfaces and for intensity by variation of the slit openings, while saturation was completely disregarded. The stimulus fields covered the visible spectrum at three or four different levels of intensity. Voeste's results may be summarized as follows:

1. All colors tend to shift in hue toward three basic hues, 560 mμ, 494–498 mμ, and 460–470 mμ. Subsequent experiments using these wavelengths as test patches show no shifts in hue. Troland suggests that these changes are due only to the Bezold-Brücke effect (42, p. 210 or 43, p. 180).

2. The comparison field is from 18.9% to 83.3% of the intensity of the stimulus field. These figures are in terms of slit openings and, of course, fail to take account of the differential sensitivity of the retina.

In 1899 and 1900 Burch wrote two papers on the production of artificial color blindness. The results of the experiments described in these papers are so unusual that extreme caution needs be exercised in interpretation. In the first paper (8, p. 2) Burch wrote, "[The method] consists of exposing the eye to bright sunlight in the focus of a burning glass behind a transparent screen of the proper colour, and keeping it there until all sensation of that particular colour is lost." This must mean, it appears, that adaptation goes to a neutral gray. Burch continues (8, p. 3), "With a combination of ruby glass . . . it takes from a few seconds to two or three minutes to produce complete red blindness." It is not clear as to whether he means the perception was of red or of neutral gray. He does claim that for an eye stimulated as described above, the sensation of red is not present, and the eye is completely blind to that hue. If this is the case, adaptation not only reaches a neutral gray, but the eye remains in that state for some time since its perception of red would be similar to its perception of gray. This condition, Burch continues, does not in any way affect the perception of other colors having no red component. It has no effect on intensity at all.

In his second paper (9) Burch attempted to show that the effect was not pathological since it could be produced by moonlight. He wrote (9, p. 217), "In producing red-blindness, as there are no colors to the left of red, I have generally used a part of the spectrum which excites only the red sensation, and which therefore must continue to appear visible at all. But I have not thought it desirable to push the fatigue of the retina far enough to destroy this sensation of light." Of course, the spectrum could appear as gray and still be visible.

Beck (4) observed the effects of intense illumination on colors and noticed the adaptation effect. He continued his experiments with spectral sources and reported that the colors were not visible as hues or were desaturated, depending on the degree of adaptation.

Porter and Edridge-Green (29) adapted the eye with a band of color for twenty seconds. Immediately after the adaptation period the eye was shifted to view a spectrum larger than the band; the band filled only the center portion of the spectrum so that comparison could be made between the adapted and unadapted portions. Although the experimental procedure is interesting, their results are erratic and their experiments cursory. They noted that the retina did not lose sensitivity for red, but sensitivity was lost for blue. In general their results follow no general form or law and there is no direct statement pertaining to the law of color adaptation.

In 1913, however, Edridge-Green (13) contributed an additional series of experiments of more significance, two of which we report:

I. After the eyes have been exposed to sodium light for twenty minutes, a spectrum is examined. The yellow has completely disappeared from the spectrum; the green and red meet without any intermediate color. But the red, orange, and green have not lost any of the yellowness which they previously possessed.

II. Blue-green spectacles were worn for ten minutes. At first all white objects appeared as a vivid blue-green, but at the end of ten minutes' time, a white object appears white. On examining the spectrum with these glasses before adaptation, there was no red to be seen, there were small amounts of orange, and the blue, green, yellow, and violet were visible. After adaptation, there were no marked changes in the orange or any other color, except the green which took on a paler and more yellow appearance. The region of the spectrum corresponding to the dominant region of the transmission of the glasses was most affected by adaptation, while the blue and yellow on each side appear bluer and yellower respectively.

The conclusions follow:

1. No color is seen by the color adapted eye unless the corresponding physical stimuli are present in the light reaching the retina. Color adaptation pro-

duces its effect by subtraction, and not by the addition of any new color sensation.

2. Color adaptation increases with time.

3. The dominant wave length corresponding to the adaptation stimulus color of spectral regions appears colorless. Colors immediately on either side of the dominant wave length are shifted higher and lower respectively.

The experimental apparatus used by Brückner (7) was Helmholtz' *Spiegelapparat* (18, p. 161). This was essentially a piece of plateglass placed perpendicular to the table. The observer is able to look through the glass to the surface of the table beyond it, or to see the reflection of the near surface of the table, or both, depending on how the glass is covered. Brückner placed an adapting stimulus composed of two parts —one half chromatic and the other half achromatic—at the far end of the table. All of the colors were from the Hering series. The chromatic colors covered the visible spectrum and are indicated in Ångstrom units. The chromatic half was either white, gray, or black. Each hue appeared three times—once with each of the achromatic colors. The reacting stimulus, placed on the near table, was one of the seven hues used as one half of the adapting stimulus or one of six additional colors. The experiments were carried out binocularly using daylight. The individual fixated the adapting stimulus through the plate glass. After ten to fifteen seconds the apparatus was adjusted so that he observed the reacting stimulus as a reflection. The results are expressed in terms of whether or not the reacting stimulus is more effected by the previous adaptation to the chromatic or achromatic stimuli. Since Brückner did not report for the case when the reaction color was the same as the stimulus color, no conclusion may be drawn except that changes in saturation do occur.

In 1920 Hubert Sheppard (33) made the first systematic investigation of color adaptation. His paper presents four main experiments:

I. Five observers who fixated in direct sunlight seven large sheets of Hering colored paper which covered the range of the visible spectrum, described the course of adaptation during fixation. In general, they report either no hue change or a hue change to yellow, with no change in intensity. The loss in saturation proceeded to a neutral gray in from 45.9 seconds to 220 seconds. When the subjects observed 180° of the same colors mixed with 180° of black and white on a color wheel, complete adaptation took place in from 23.4 seconds to 121.3 seconds.

II. In the second experiment, circles of the Hering papers placed in a field of Hering gray were illuminated by a 75 watt bulb. By means of a telegraph key attached to a kymograph the observers signaled when important perceptual changes occurred and also when uncontrolled eye movements occurred. They did not give a running account of the

adaptation process. In general, adaptation was not characterized by shifts in hue as in the first experiment nor was complete adaptation reported by all subjects. All observers, however, reported loss in saturation and told of a cloud-like fog which moved over the color. The adaptation times (times at which no further change occurred) ranged from 40.3 seconds to 120.2 seconds. Sheppard determined the chromatic limens for each of the stimulus hues by the method of limits using a disc containing black, white, and the hue. The limen has a negative correlation with the adaptation time. Wherever the adaptation time is long, the chromatic limen is small.

III. In this series a remodeled spectrophotometer so designed that the subject, observing through an artificial pupil, was able to perceive a small portion of the spectrum as a disc, was used. Five positions were chosen from the visible spectrum. All but a single observer reported complete adaptation. The time ranged from 76.6 seconds to 169 seconds. Since Sheppard noticed that these times formed a curve which closely resembled the brightness curves for spectral colors, he performed another series in which the colors were equated for brightness value. The resulting figures, although more similar than those obtained before the brightness values were equated, still showed another factor to be operating. This, Sheppard reasoned, was the chroma value already demonstrated as being related to the adaptation time in his second experiment.

An additional series was conducted in which the chroma values were made equal (by subjective techniques). It does not appear that the brightness values were kept constant as well. His apparatus was too crude to control both factors at the same time. The adaptation times (for complete adaptation) for the various colors were now close enough so that Sheppard considered chroma as being the chief determiner of adaptation times. Introspective reports indicated no hue change except for red and violet which shifted toward yellow and blue respectively. There was no reported change in intensity.

IV. The last experiment was an attempt to determine whether complete adaptation times could be reached with very high intensities. A carbon arc provided the light source while gelatin filters were placed over a pair of glasses. The experiment was monocular. The subject fixated the arc while wearing the glasses until no further change took place. He then fixated a spectroscope which was equipped with a powerful illuminating source and a rheostat to control the intensity. (The rheostat no doubt changed the color temperature.) This served as a comparison light. The subjects reported complete adaptation in from 105.3 seconds to 197 seconds. There was some shift in hue toward yellow but most subjects reported no shift in hue at all. As Almack points out (2, p. 20 ff), Sheppard's description of this portion of his experiment is not clear. It is difficult to determine the nature of this comparison light formed by the spectroscope.

Troland (41) has reported the results of what he terms photopic adaptation. These are the same experiments referred to in his previous papers (37, 39, 40). The eleven experiments will be described:

I. In the first experiment, designed to test the equilibrium sensation for direct sunlight on white paper, a large white paper was fixated in direct sunlight for ten minutes. He concluded that prolonged exposure of the retina to large, very intense, achromatic stimuli does not involve a reduction of the sensation to neutral gray. The luminosity of the perception after adaptation is much greater than that of mid-gray. Further, the equilibrium sensation just described is brought about rapidly.

II. In this experiment large sheets of colored paper, selected to cover the range of the visible spectrum, were observed in direct sunlight. From these experiments he drew the conclusions that after exposure of the retina to large chromatic stimuli of high intensity, the resulting perception is not one in which the hue of the stimulus has completely disappeared. There may be some shift in hue but the perception is never one of neutral gray. The equilibrium point is reached soon after fixation.

III. A dark gray square, placed in the center of a yellowish-green Hering paper, was fixated until the green component of the paper faded leaving a light blue. At the end of four minutes a shutter arrangement substituted a Hering violet paper for the gray square. The outlying field became a darker blue while there was no reappearance of the green. But on the introduction of a white object into the green adapted area, the green color returned. Additional tests indicate that this phenomenon is not due to the contraction of the pupil. The experiment, of course, lends support to the theory of central control of color adaptation.

IV. Observations similar to those of Experiment I were made with stimuli consisting of two juxtaposed Hering papers of different hues. One paper filled the upper half of the field while the other filled the lower half. The six possible combinations of the four *Urfarben* were employed. Troland concluded that the principle of simultaneous color contrast applies to the equilibrium position. The introduction of a neutral object into the bi-chromatic field brings about the same change in quality for both halves of the field which was described in the previous experiment for a simpler stimulus. An experiment using a bipartite fixation of black and white failed to show any tendency of the field to approach neutral gray.

V. This experiment concerns the perception of a small luminous spot on a dark background. If the illumination is low, the spot tends to disappear and remain below the visibility threshold or fluctuate between visibility and invisibility. As Marbe (25) demonstrated, the more intense the spot, the less is the tendency for it to disappear. This was confirmed by Troland, and with spots of colored light fixated monocularly at high intensities, observers reported that the stimulus colors

changed as in the previous experiments, but it did not approach a neutral gray nor did the spot fluctuate.

VI. The fluctuations of the last experiment are explained by supporters of the Hering theory as being due to eye movements permitting the retina to recover. See, for example, Ferree (15). A large sheet of Hering paper was fixated. The paper was large enough to be well out of the field of possible eye movements. With steady monocular fixations, repeated fluctuations in the field could be observed. Temporary disappearance could be obtained.

Fluctuations described here cannot be explained on the basis of adaptation followed by recovery due to eye movement. Deviations in fixation would move the image on the retina through not much more than one fiftieth of its diameter. Changes in accommodation could not be responsible for fluctuations since such changes would only be on the edge of the image.

VII. This experiment was designed to investigate whether the changes in the iris can be responsible for the visual fluctuations which the previous experiment showed were not due to eye movements or accommodation. The stimulus surface described in the previous experiment was perforated and a telescope placed behind. The iris of the observer was illuminated so that changes could be observed through the telescope. The observer pressed a telegraph key when the colored spot was seen to disappear. The pupillary contractions and expansions have a perfect correspondence with the reported fluctuations.

VIII. If changes of the iris are responsible for fluctuative disappearances, it is obvious that the elimination of these changes by the use of drugs such as pilocarpin or atropin or the use of an artificial pupil should cause the elimination of these fluctuations. Drugs have been used on similar problems by Pace (28) and McDougall (27). Both investigators report that fluctuations were present. For the effects of the artificial pupil, see Troland (38). The intensity of the colored spot stimuli was placed under the control of the subject. Starting with a very low intensity, he increased the brightness until no fluctuations occurred for three minutes. The stimulus was then regarded as being at the required threshold. The results indicate that if the action of the iris is eliminated, no fluctuations occur for long intervals. The fluctuations which do occur, according to Troland, are due to mechanical difficulties of the artificial pupil, to the electrical condition of the visual system, binocular rivalry, etc.

IX. Spots of various colors equated for brightness were fixated with the artificial pupil. Since the brightnesses were of moderate intensity, no fluctuations of the spot stimuli were observed. The adaptation did not proceed to a neutral gray. If the stimulus is a psychological primary, there is no essential shift in hue. Intermediate hues tend to lose the red and green components.

X. The subject observed a colored spot for twenty seconds. The intensity of the spot was then decreased until the subject indicated that it had disappeared. Fixation was kept constant by means of the bright crescents appearing at the edges of the adapted area. Reappearance of the whole area always occurred.

XI. In the greater number of visual studies it is necessary to stimulate but a single eye while the other is blindfolded. This is an ideal condition for binocular rivalry. To demonstrate this phenomenon, one eye observed a colored spot. Immediately after fixation, the stimulus was transferred to the other eye where it appeared at first in its proper color. But soon it became a sparkling white, reminding one of binocular luster. It is therefore suggested that the after image appeared in the other eye and when mixed with its complement (the stimulus hue), it appeared white.

Troland did not discover this phenomenon. It was first noted by Newton in a letter to Locke and is the subject of Titchener's thesis. See Titchener (**35**) or for a brief discussion in English (**36** p. 49 ff). See also Franz (**16**, p. 44) for evidence that this effect does not occur.

Troland and Langford reported in abstract form some experiments concerning chromatic minuthesis (**44**). Minuthesis is their term for adaptation. They contend that changes which occur are only changes in saturation. Any further changes in hue are due to the Bezold-Brücke effect.

In 1923 Hamilton and Laurens (**17**) used a comparison technique to study adaptation. One eye was fatigued for thirty seconds. Then both eyes were placed at telescopes which presented a colored field to each eye. The fields were adjusted until a just noticeable hue discrimination was indicated. The results were as follows:

1. Red, green, and blue have a selective effect when used as stimulating sources. All other colors do not.
2. During adaptation brightness is reduced.

One of the more important studies of chromatic adaptation was performed by Almack (**2**) in 1928. Her problem was to measure the loss of sensitivity of the color adapted eye. Spectrum lights, used as stimuli, were controlled with respect to hue, saturation, and intensity. The threshold of the spectral light which was used to induce loss of sensitivity was determined at the end of each adaptation period. This was accomplished by the introduction of a motor rotated sectored disc into the path of the stimulus light. The rotating disc could be removed at will. Determinations were made after varying amounts of time (from 2 to 300 seconds) and gathered together in the form of curves. Com-

parison could be made between different intensities of the same hue, but not between different hues since the thresholds for the different wave lengths have different values. Comparison is made, however, when the determinations are reduced to a percentage basis. The experiments were made with both a dark adapted and light adapted eye. Almack drew the following conclusions from her work:

1. For stimulus lights made equal in intensity, the sensitivity of the non-adapted eye is from greatest to least, yellow, green, blue, red. Initial loss in sensitivity is greater than at any other period. Initial sensitivity is greater for the dark-adapted eye than for the light-adapted eye, but both sensitivities tend to become equal with time. The rate of adaptation increases with the intensity of the stimulus.

2. For lights equalized for perceived saturation of the stimulus, the rates of adaptation to the various stimuli are very unequal. The largest rate (2, p. 115) "of adaptation by no means occurs at that intensity of light which gives maximum saturation to the color sensation. . . . The rate of adaptation is a function of the physical intensity of light, not of its subjective aspects, saturation and brightness."

3. For lights made equal with respect to wave length, the loss of sensitivity varies with the wave length. Under dark-adaptation, chromatic sensitivity is greater than for light-adaptation.

There is some confusion as to just what is measured by the technique described in this experiment. Consider a retina adapting to a stimulus light of a particular hue, saturation, and intensity. After a time, we determine the intensity limen of the eye to the same hue and saturation. This is true since we use the same source and introduce a rotating sectored disc. If we consider the color solid to be a double cone, then in dealing with hues of maximal saturation which lie on the surface, the saturation of such hues is dependent on intensity. They are dependent variables. Then if the intensity of our stimulus light is reduced, we have also reduced the saturation. On the other hand, if the color solid is considered to be a cylinder (and this is implicit in the 'fundamental sensation curves' of König and Dieterici (22)) there is no relation between saturation and intensity. Under these conditions, measurements by the above sectored disc can only indicate changes in perceived intensity.

Roaf (30), too, used the threshold of the eye as a measure of sensitivity. His apparatus was very complicated, but essentially it permitted the threshold to be determined after stimulation. He concluded that when any light stimulates the retina, the sensitivity, even of another part, is decreased to the short wave lengths and somewhat less to the long.

Kravkov (23) utilized the Helmholtz color mixer (18, p. 395 ff) in

1928 to test a theory of Lasareff concerning adaptation. The left field was fixated for ninety seconds and then matched with the right. The fixation was monocular. The match was made for intensity only. The results, claims Kravkov, support the outlined theory and indicate curves described by an equation with variable parameters. Kravkov's curves are, however, misleading. For example the first curve on page 93 (23) would fit the observation points just as well if it were completely inverted, or, indeed, if just a horizontal line were drawn. The conclusions to which Kravkov came are the following:

1. The decrease of intensity of a color sensation following color adaptation is of great importance and accords a lessening of the stimulus to 50% of its initial value.
2. The course of adaptation depends not only on the intensity of the stimulus but also on the hue. At the same intensity, violet (451 mμ) is more quickly adapted than red (656 mμ) and less than green (550 mμ).

Helson and Judd (19) performed two important experiments in 1932. The first concerned itself with eye movements and their relation to color adaptation. An apparatus was constructed so that an individual placed his head in a sphere 36 inches in diameter. The inside of the sphere was lined with orange-red Hering paper. The subject fixated the inside of the sphere binocularly. Strong illumination eliminated pupillary changes. In theory the apparatus was so constructed that any eye movements did not change the effective stimulus. Observations were made for from five to seventy-five minutes. The conclusions arrived at by Helson and Judd by use of their 'adaptation sphere' are as follows:

1. The neutral condition of gray did not appear permanently. Gray was perceived but its perception was cyclic and spasmodic. They think that this was probably due to the pupillary effect.
2. The red component vanished making the perception more yellow.
3. Most of the adaptation took place within the first few minutes of stimulation.

In the second experiment, the subjects wore strongly selective red or green glasses (the transmissions are given in detail). The observer wore these glasses for as long as five hours and at hourly intervals identified colored Hering papers for hue, saturation, and intensity. It is difficult to understand how a subject wearing glasses transmitting nothing less than 600 mμ could see the blue component in a blue paper. Note this report (19, Table 2, p. 388). The results are as follows:

1. After five hours, the lighter colors appeared in the hue of the glassse while the darker colors were either neutral gray or complimentary in hue to that of the glasses (because of the after image).

2. At first there was a sharp loss in saturation as well as a shift in hue—a shift toward yellow.

Attention should be called to the theoretical explanations in this paper. Helson and Judd assume that if complete adaptation did occur while the individual is wearing colored glasses, he would see everything in 'natural' colors. This, they point out, is impossible if the glasses are highly selective since adaptation cannot supply what is physically not present. Further, they continue, if the glasses are not highly selective, the objects may appear in 'natural' colors since the predominant wave length will cause quick adaptation to itself and since the glasses transmit at least some of all the wave lengths, objects will be seen in 'natural' colors. This last is, of course, quite possible. Such non-selective glasses might lead to such a result. But adaptation with highly selective glasses can occur, at least theoretically. In this case everything transmitted would eventually be perceived as a neutral gray; perceived as a gray physical stimulus would be perceived by a non-adapted eye.

In 1934 Bouma (6) commented that when looking at a white surface with a black card at the center with sodium (or neon) light, the black part, after a time, took on a violet (or green) hue. His study, however, was more concerned with after images.

The first approach to the color adaptation problem by use of twin colorimeters was by Wright (47) in 1934. The apparatus was that described by him in (46). It can be characterized by the following. There were two eye pieces each of which was connected to a Wright colorimeter. Any hue could be seen in either of these eye pieces by adjusting three knobs associated with each colorimeter. The knobs mixed three spectral primaries. All hues in high intensities but not all saturations could be produced. The third eye piece, to the right of the other two, presented a field of white light. The subject was dark adapted for a period of thirty minutes. He then fixated the fields of the two colorimeters; the left field was seen by the left eye while the right field was seen by the right eye. Binocular rivalry did not occur, according to Wright. The test patch of the right eye was matched with the comparison field of the left. The calibration readings were noted by the experimenter. The subject then shifted his right eye to the eye piece showing white light and fixated this patch for three minutes. The left eye, during this time, was unilluminated. At the end of three minutes, the observer again fixated the colorimeter fields as before. The fields no longer match. The observer quickly matches the left patch with the perception of the right. When the match is completed, he signals the

experimenter who notes the time and the wedge readings, and the subject proceeds to make another match. The process is continued until no further change is noted.

This attack of the problem is somewhat different from anything else we have encountered. Here, it is the recovery time after adaptation that is measured. And recovery is given in terms of the spectral primaries of the colorimeter. At the beginning the two fields of the colorimeter are the same. The right eye is stimulated with white light which, it is assumed, depresses (or adapts) the activity of the receptor for the test patch to a level different from the original perception. The eye is then allowed to recover to the level of the test patch. This recovery is supposedly measured with the left eye. What is not taken into account, however, is that the eye does not recover to the level set by the test patch but continues to be stimulated by it until the receptors reach the equilibrium position. It appears that the white light is a confusing element and we wonder that Wright did not adopt the experimental procedure used in Cohen's (11) experiment. For additional papers using retinal recovery as a measure of adaptation see Allen (1) and the bibliography included in that paper.

Since the results of Wright's experiment are given in specialized response curves, no interpretation can be given until they are mathematically transformed to a more widely used system of primaries.

Color fatigue of the peripheral retina was investigated by Cogan and Cogan (10) in 1938–39. A perimeter was employed to hold the colors. The test color (Hering paper) was framed in a background having the same saturation as the test color. The time between the first perception of the color and its eventual merging into the background was considered to be a measure of adaptation. The results indicated the following:

1. Color adaptation continues to a neutral gray. The rate of adaptation increases toward the peripheral field.
2. Green was the most easily adapted color while red was the most difficult.
3. Dark adaptation influences color adaptation times.

There is a cursory study by Schouten and Ornstein (32) published in 1939. Their experiments utilize a separate field for each eye. They report that it was difficult to keep the two fields from fusing or overlapping. The comparison could be made in brightness only. Their contribution is that the adaptation of a test patch (here loss in intensity) is dependent upon the state of the rest of the retina.

Cohen (11) has recently completed an investigation in which he

utilized an instrument which presented two test patches to a binocular subject. The apparatus was so constructed that any hue could be placed in each patch by the adjustment of three controls. A test patch was placed in the right side of the instrument, a neutral gray on the left. After one minute the observer matched the left with what he saw on the right. The match being made, the left side is returned to gray while the right continues to adapt. After another minute, another match is obtained. In this way successive matches are made against time for a particular test patch. The instrument readings which are given in terms of monochromatic radiations of red, green, and blue (700 mμ, 546 mμ, 436 mμ) are transformed to the monochromatic system of hue, saturation, and intensity. Thus *quantitative* changes in these latter variables were plotted against time. He did not employ intermediate colors.

Under the conditions of Cohen's experiment the following three laws may be stated:

Law I. The course of color adaptation consists of a gradual but never complete loss of saturation, with no change in hue, and an increase in intensity.

Law II. The rate and degree of desaturation depends upon the hue, the relative intensity, and the relative saturation of the stimulus color.

a. The loss of saturation for a green stimulus is most rapid, it is less for red, and least for a blue stimulus.

b. The less intense the stimulus, the greater the loss of saturation.

c. The less saturated the stimulus, the more rapid is the initial rate of desaturation; the equilibrium position, or point of lowest desaturation, is independent of the saturation of the stimulus.

Law III. The rise in intensity is dependent on the saturation and intensity of the stimulus and independent of the hue of the stimulus.

a. If the saturation and intensity of the stimulus are both high or both low, the rise in intensity will be greater than if one is high and the other is low.

Summary of Methods

The methods used by the various investigators may be summarized as follows:

I. Introspective—mere description of adaptation.

De La Hire (12), Aubert (3), Ssamajlow (34), Sheppard (33), Troland (41), Helson and Judd (19), Bouma (6)

II. *Controlled introspective—perceived objects are carefully controlled (no comparison with unadapted portion of retina).*
Bokowa (5), Burch (8, 9), Beck (4), Edridge-Green (13), Sheppard (33), Helson and Judd (19)

III. *Liminal–limen (intensity) is measured after adaptation.*
Almack (2), Roaf (30)

IV. *Comparative monocular introspective—two fields viewed monocularly—differences are noted introspectively.*
Exner (14), Porter and Edridge-Green (29)

V. *Comparative monocular non-introspective—two fields viewed monocularly—match made on instrument.*
Exner (14), Schön (31), Hess (20, 21), Voeste (45), Edridge-Green (13), Kravkov (23)

VI. *Comparative binocular introspective—two fields viewed binocularly—differences noted introspectively.*
Brückner (7)

VII. *Comparative binocular non-introspective—two fields viewed binocularly, one field for each eye—match made on instrument.*
Wright (47), Hamilton and Laurens (17), Schouten and Ornstein (32)

VIII. *Comparative duo-binocular non-introspective—two fields viewed binocularly, both fields seen by both eyes—match made on instrument.*
Cohen (11)

Summary of Results

The results obtained by the various investigators may be summarized as follows:

I. *Saturation*
Partial loss: Bokowa? (5), Aubert (3), Exner (14), Troland (41), Helson and Judd (19), Almack? (2), Roaf? (30), Cohen (11)
Complete: Edridge-Green (13), Sheppard (33), Cogan and Cogan (10)

II. *Intensity*
No change: Burch (8, 9), Sheppard (33)
Loss: Schön (31), Voeste (45), Almack (2) Hamilton and Laurens (17), Roaf (30) Kravkov (23), Schouten and Ornstein (32)
Increase: Cohen (11)

III. *Hue Shift*

	*Basic Colors**				
	R	Y	G	B	V
Investigator					
Bokowa (5)		x		x	
Exner (14)	656–589		527–517		431
Hess (20, 21)	650	565		490	
Voeste (45)		560		498–494	470–460
Sheppard (33)		x		x	
Troland (41)	x	x	x	x	
Helson and Judd (19)		x			
Cohen (11)	no shift				

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* Basic colors indicate those hues which do not change and toward which all other hues shift. An x indicates an investigator concluded that a particular basic color is present. In some cases the notation is in millimicrons.

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THE EFFECTS OF NOISE*

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The development of modern industrial society with its concentration of population and machinery in confined areas has brought with it a concomitant concentration of noise. The popular science literature of the past twenty years is dotted with articles describing the deleterious effects of noise in highly emotional terms unwarranted by the facts. Few problems have attracted a wider range of professional interests than the problem of noise and what to do about it. Physicians, public health authorities, architects, psychologists, otologists, physicists, sound and electrical engineers have contributed to the literature. On the industrial side interest and concern is evidenced by the telephone and radio industries, automotive, airplane and railroad equipment designers, manufacturers of office equipment, building materials and a wide range of consumer appliances. Most recently the safety engineers and insurance companies have expressed an interest because of the growing recognition of occupational deafness. One is reminded also of the various noise abatement campaigns which sprang into prominence in the early 1930's.

In the light of this wide spread interest, one might reasonably expect to find a considerable body of well grounded scientific literature relevant to the topic of noise and how it affects the behavior of man. On the contrary there is a paucity of studies dealing with the problem at the human level in terms which provide a basis for generalizations even though a wealth of unsystematized experience by acoustical engineers points to the conclusion that people are benefited by noise reduction. Part of the reason for this state of affairs lies in the fact that only within the past 20 years has there been any attempt to standardize terminology and techniques in the measurement of noise itself. Even today the available procedures are still in the developmental stage where many improvements are to be desired.

It is the purpose of this review to summarize critically those studies pertaining to the effects of noise on human beings. Occasionally it may be necessary to refer to studies of animals to point up certain areas but in general the emphasis will be upon studies using human subjects.

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Consideration will be given in turn to the effects of noise on output speed and accuracy, fatigue and energy expenditure, the course of noise-adjustment, stimulation deafness, and finally the factors determining relative annoyance. Preliminary to an examination of these problems it is necessary to summarize the methods of measuring noise and their limitations.

Definition of, and Procedures in Measuring Noise

Noise has been defined as unwanted sound (60). This statement bearing as it does a heavy value judgment appears to be widely accepted among physicists, engineers and the like. Paradoxically the psychologists lean toward a definition couched in more physical terms. Thus Dockeray (11) writes, "If a complex of vibrations is not in a harmonic ratio, we speak of the result as noise." And Dashiell (7) agrees in saying, "When air vibrations are non-periodic and irregular, or are less than two full vibrations, they produce *noises* rather than *tones*." The present paper is primarily concerned with noise as unwanted sound recognizing at the same time that the degree of annoyance may vary from a theoretical zero to some maximum point depending upon factors to be enumerated later.

The unit of noise measurement most widely used is the decibel. This is a logarithmic unit based on sound energy measurements. Two sounds differ by N decibels when $N = 10 \log_{10} E_1/E_2$. (E_1 = sound energy density of sound 1; E_2 = sound energy density of sound 2.) In practice it has been found that 1 decibel represents *approximately* 1 J.N.D. in a 1000 cycle tone. The zero point on the decibel scale could be set at any value but has now been standardized at 10^{-16} watts per square centimeter which is a convenient value somewhat lower than the normal threshold of hearing for a 1000 cycle tone. Publications prior to the tentative adoption of standard accoustical terminology in 1936 were often confusing in their use of the decibel scale because the minimal auditory threshold varies with frequency of vibration. Early writers refer to a tone of 200 cycles at 30 db without specifying whether they mean 30 db above the 1000 cycle tone threshold or 30 db above the 200 cycle threshold.

Less frequently used in this country is the word *phon* which is numerically equivalent to a decibel when considering the standard 1000 cycle tone. For any tone other than a 1000 cycles its phon value is determined by equating in loudness that tone with the standard. Referring to Figure 1 this means that a 300 cycle tone having an intensity level of 40 db. above 10^{-16} watts per square centimeter will appear to

be equal in loudness to a 1000 cycle tone having a 30 db. intensity level above the same base. Hence the 300 cycle tone at that intensity has a loudness level of 30 phons.

Related to this unit are the terms *loudness units* and *sones*. If a 1000 cycle tone at 40 db. be taken as a standard and a second tone be reduced to one-half the subjective loudness of the standard, the second is said to

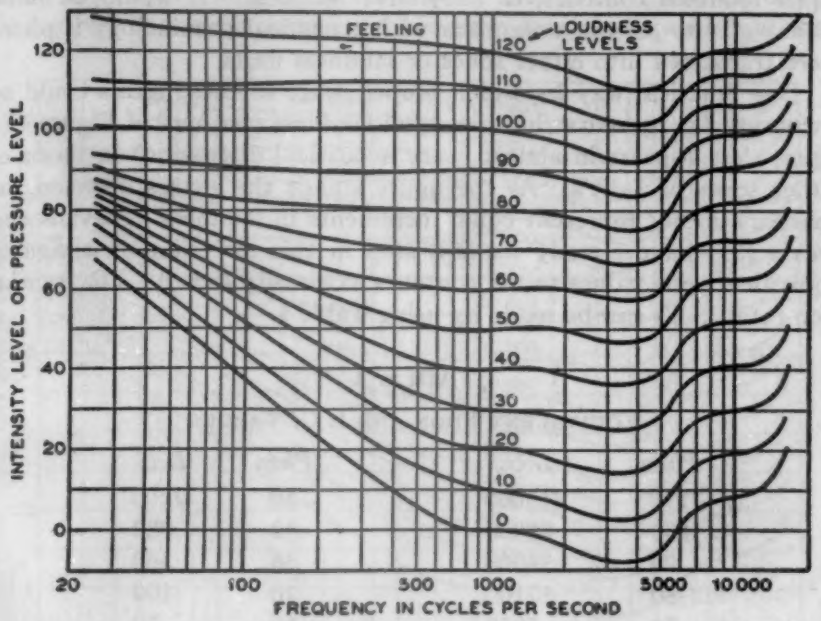

FIGURE 1. EQUAL LOUDNESS CONTOURS (30)

be one-half a sone. Conversely a tone adjusted so that the normal observer judges it to be twice as loud as the standard is said to be 2 sones.

Stevens proposed this latter unit and has published curves (51) showing the relation of sones to db. level for a number of representative frequencies. The sone curve for tones between 700 and 4000 cycles is almost identical with a curve of loudness units (L.U.) published by Fletcher (16) and subsequently adopted with slight modification by the American Standards Association (60). The L.U. curve was constructed on the basis of several independent studies conducted in this country and in England where estimates of various fractional and multiple loudnesses were plotted against phons. The chief difference between the L.U. scale and the sone scale is in the magnitude of the numerical

values assigned, the L.U.'s being roughly 1000 times greater than sones. Either scale is more indicative of the subjective effects of noise level changes than the db., phon or the direct intensity scales. Although the American Standards Association has specifically defined decibels, phons, and loudness units, it is common practice at least in speaking and often in writing to use the term decibel to cover physical sound intensity, equal loudness contours, or subjective loudness. It would be more meaningful to persons unfamiliar with acoustical terminology if phons were translated into either sones or loudness units.

One practical way by which proper usage of these terms could be encouraged is by redrawing the equal loudness contours of Figure 1 (a figure that appears in almost every acoustical discussion) in terms of either sones or L.U.'s. As the figure stands the spaces between the contours do not represent equal increments in loudness. Stevens and Davis (51) have already made a step in this direction by assigning equivalent sone values to the contours as already drawn. The translation into L.U.'s can be made by using Table I.

TABLE I

EQUIVALENT PHON AND L.U. VALUES

Phon	*L.U.*	*Phon*	*L.U.*
110	215000	50	2200
100	88000	40	980
90	38000	30	360
80	17100	20	100
70	7950	10	10
60	4350		

Figure 2 is an attempt to make the equal loudness contours more meaningful by translating phons into L.U.'s ($\times 10$) using the table published by the American Standards Association (60) and then plotting the contours so that each successive contour going up the scale represents a doubling of the loudness.

Two basic methods have been employed in measuring the loudness of sounds. The *first method* or older of the two is the binaural matching method with or without an off-set receiver. In this procedure a normal observer is provided with a receiver being driven through a calibrated attenuator by some kind of oscillating circuit. While listening to the sound to be measured with one ear he matches the loudness in the other ear by adjusting the attenuator. The attenuator has usually been cali-

brated in terms of db. above the threshold for a 1000 cycle tone. A variation of this method employed in some of the earlier work was the use of a phonograph record of a warbling tone. The gain control was calibrated in decibels above the hearing threshold.

Some question has been raised regarding the advisability of using a receiver, offset from the ear a centimeter or more, for the purpose of mixing the known and unknown sounds. Those favoring the use of an offset argue that comparisons are made more accurate because both the

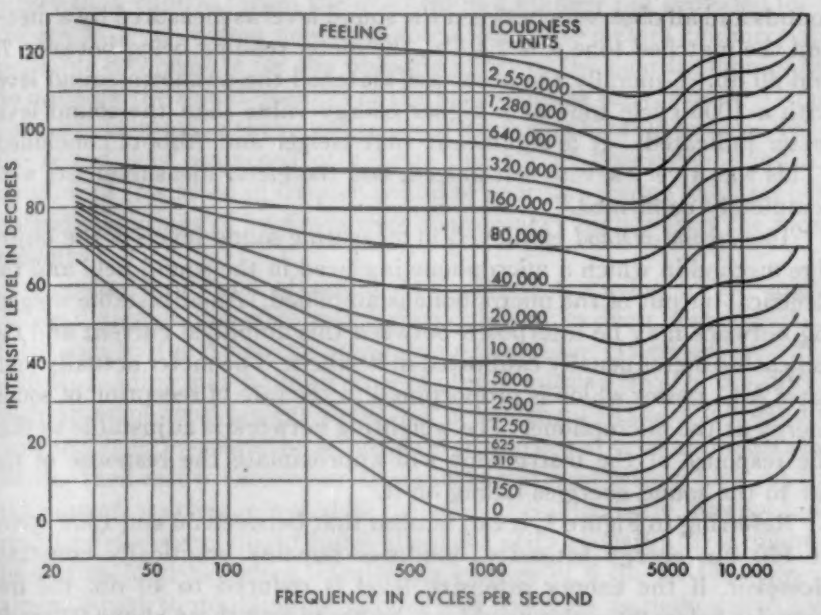

FIGURE 2*. EQUAL LOUDNESS CONTOURS EXPRESSED IN LOUDNESS UNITS (×10)

known and unknown sounds are matched in a single ear. On the other hand others (5) insist the offset has a baffle effect on the incoming waves from the unknown source distorting their loudness. Practiced observers following a rigid variant of the method of limits familiar to psychophysicists have in this manner measured complex sounds with a fair degree of reliability. "The spread of the mean final settings normally does not exceed ±2 db. at 100 db. level, ±4 db. at 80 db., and ±6 db. at 50 db." (5). Some experience using a receiver without an offset while

* In preparing Figure 2, loudness units were multiplied by 10. To reduce, neglect the final zero, i.e. 2,550,000 = 255,000 L.U.'s and so on.

matching the loudness of an unknown noise indicates that the position of the receiver is highly critical. Slight pressure on it or a small shift with respect to the meatus produces large changes in the apparent loudness of the calibrated sound.

Using a different arrangement Geiger and Abbott (20) presented twenty observers with sounds from a variety of sources, (buzzer, rattler, vacuum cleaner, bell, etc.) by means of a microphone, amplifier, and loud speaker which were to be matched in loudness by a 1000 cycle tone introduced also through a loud speaker. The mean difference for all sounds and all observers between the sound level as measured by a meter and the matched tone was 2.4 db., the meter reading being between 76 and 40 db. Generally the observers matched the unknown sound level with a 1000 cycle tone at a higher energy value than the sound level meter indicated. It is significant that Geiger and Abbott concluded, "This was a very severe test indeed, and the meter measurements were completely confirmed."

The *second method* employed in measuring sound levels is the objective method in which a microphone is placed in the sound field and the electrical output of the microphone is amplified. An adjustable weighting network may be interposed between this amplified current and the indicating meter usually calibrated in decibels. The meter actually indicates A.C. power which is proportional to the rate of reception of sound energy at the microphone. The weighting network is adjustable so that the response of the instrument will approximate the response of the ear to the sound energies falling on it.

Referring to Figure 1, it can be seen that between 30 and 1500 cycles at 100 db. energy intensity, loudness remains practically constant. However, if the energy intensity level is reduced to 40 db. the frequencies below 90 cycles would not be heard and those above 90 would vary from just perceptible to 40 phons in loudness. Consequently a noise meter must be adjustable so that at high intensities it gives a reasonably constant output for all frequencies and with low intensities it should give little or no response at low frequencies, but should increase its output as the frequency is raised to approximately 2000 cycles and then decrease as the frequency exceeds this level. Since it is not at present practically feasible to build a portable meter with networks permitting response curves similiar to each of the loudness contours most commercial sound-level meters are provided with three such networks adaptable to levels approximating 40, 70, and 100 db. in over all intensity.

Under special conditions where the noise to be measured is made up

of a number of harmonic frequencies as is the case with the noise from large transformers the objective method gives readings that may be as much as 30 db. *lower* than the aural comparison method (5). The reason for this lies in two related facts. First, frequencies close to, but not exactly equal to the harmonics of a given fundamental exert a masking effect on that fundamental thereby reducing the loudness which would otherwise be present. Second, when a number of harmonic components are present in the noise being measured their energies add to, rather than subtract from the loudness in a manner not provided for in the sound-level meters now available (56). The meters commercially available compensate for the masking effect of the near-harmonics but do not sum the sound energies of the harmonics as the ear does. Geiger and Abbott noted in the report referred to above (20) that in cases where the sound was composed of a large number of "component musical notes, well separated in frequency and of nearly the same loudness" the individual ratings of loudness were considerably higher than the meter readings.

Another limitation of sound level meters traceable to the microphone design is the wide tolerance permitted in their response characteristics by the American Standards Association. These are such that a 90 db. reading may have an error of plus or minus 3 db. On the surface such an error may appear small yet if one should take a low reading instrument to measure the sound level in a given area of approximately 90 db., then return later after acoustical treatment with a high reading instrument, no change would be registered. Subjectively, however, unpracticed observers might judge the noise after acoustical treatment to be half as loud as before. Indeed, a change of 6 db. at that level is almost equal to doubling or halving the loudness as determined by a number of independent investigators using trained observers under careful laboratory conditions (19, 24, 26). In many field measurements of noise these errors are small relative to the fluctuation in sound energy from moment to moment and place to place.

It becomes clear that the aural comparison method is superior to the objective method is some instances provided that competent observers are employed, and the test tone is properly calibrated. The known variation in individual observers are of the same order as the allowable tolerances in microphone response characteristics. In addition, however, the sound-level meters even if accurate in their response to the sound energy of the field do not register the non-masking effects of harmonic components in a sound spectrum. Undoubtedly one of the chief reasons for the wide use of sound level meters rather than the aural

comparison method lies in the fact that the meter readings are repeatable with a high degree of reliability. Furthermore, it is difficult if not impossible for an observer to match sounds varying greatly in intensity from moment to moment. The aural comparison method does show differences in loudness when the same observer makes repeated observations of the same sound. This is especially true of inexperienced observers and leads investigators after a brief trial of the method to discard it. On the other hand the work of Geiger and Firestone (21), Ham and Parkinson (23), and Fletcher and Munson (17) showed that practiced observers following carefully a standard procedure are remarkably reliable in their estimations of loudness of relatively steady tones.

It is possible to compute the loudness of a complex noise by the use of a wave analyzer which records the magnitude of sound energy in each of several wave-frequency bands. The noise spectrum thus determined provides the basic data for computing the loudness using formulae developed by Fletcher and Munson (17). The method is cumbersome and yields results only slightly different than the mean value of several observers using a properly calibrated test sound.

It becomes clear that both methods of measuring noise levels requires more than an ability to set up a microphone, flick switches and read dials. The meter readings must be interpreted in the light of the nature of the sound spectrum and the known tolerances of the meter itself. On the other hand the aural comparisons must likewise be conducted and interpreted by one familiar with the standard manner of listening and the known sources and magnitude of errors. In the hands of competent investigators either method will not give a measure of high precision but will provide a basis for an informed estimate of loudness.

Effects on Production

One may find in textbooks references to a number of reports indicating that production is markedly increased with a reduction in noise. Several of these emanate from an anonymous article (62) in which references to the original reports are lacking. It is not possible, therefore, to evaluate the claims that "lowering the noise level in a telephone-exchange room from 50 db. to 35 db. resulted in a forty-two per cent reduction in errors and a three per cent reduction in cost per message" (4). Or that "moving the assembly department of a temperature-regulator company from next to the boiler room to a quieter location reduced faulty assemblies from seventy-five per cent to seven per cent of those turned in for inspection" (4).

Only one field study in this country has been completed with sufficient control and covering a sufficient time to place confidence in the results. This study has not been formally published in detail although it has been referred to frequently (32, 59). The Aetna Life Insurance Company in December 1928 installed sound absorbing materials in an office occupied by typists, clerical checkers, punchcard and comptometer operators. Bonus figures were available reflecting the efficiency of employees for a year prior to installation. This "efficiency" refers to the allowed time for a given amount of work as determined by time studies, divided by the actual time taken. The average difference in the bonus based on efficiency of fifteen clerks taken at semi-monthly intervals for the year prior to, and a year after acoustical treatment was 9.2% in favor of the quieter condition. At no time during the "quiet" year did the bonus go below the level of the first year. No other changes were introduced into the working conditions during these two years. The sound level as measured by a Western Electric 3-A audiometer using the loudness matching method was 35 "sensation units"* after acoustical treatment.

A control check on these findings was made after one year by covering the sound absorbing materials with gypsum board. The sound level was then 41 "sensation units." The bonus efficiency immediately dropped to an intermediate value between the two previous years, but within two months was approximately equal to the level of the "quiet" year. At the time the bonus reached this high level in the presence of the greater noise there was a reduction in work, only the more efficient employees being retained from then on, which probably accounted for the high average efficiency. Comparable, but less clear-cut results were noted among the punchcard, and comptometer operators. In these latter groups machine breakdowns and other uncontrolled factors complicated an interpretation of the data.

The available data provides no basis for the calculation of statistical tests of reliability of the differences noted. In spite of this fact it is likely that the advantage noted in favor of the quieter condition is not a matter of chance.

The influence of noise in school children and grade school work was investigated by Obata (39). The noises were a variety of phonograph records supplemented by a mechanical rattler varying between 60 and 85 db. With each type of sound working speed was more affected than accuracy in arithmetic and symbol cancellation. The adverse effects

* This is a unit roughly equal to a decibel and used prior to 1936.

were not solely related to intensity since certain soft records were as distracting as the rattler.

At the Waverly Press, Baltimore, a number of changes were made in working conditions in 1935, among them being an acoustical installation. It was reported that, "The reduction in noise and the use of rest periods are found to be useful means of producing not only higher efficiency from the standpoint of management but also a much more agreeable situation for workers themselves" (48).

In an attempt to duplicate the industrial production situation Laird (29) devised an "experimental factory" in which the subjects were required to touch a metal plate with an electrical stylus through 10,500 small holes per work spell as they appeared in a moving tape. The holes were irregularly spaced and of irregular size appearing in a window 3½ by 1½ inches directly in front of the worker. Production of four subjects, throughout each of the 4½ hours work spells was recorded by electrical counters. The results presented in the form of the number of holes missed per working spell per worker showed almost a straight line drop from 400 holes missed at 90 db. to 300 missed at 40 db. when the complex noise delivered through an amplifier by the Western Electric 3-A audiometer was presented. Using pure tones at loudness levels equal to a 512 cycle tone at 60 db. on the Western Electric 2-A audiometer, errors increased slightly as the frequency was stepped up from 64 to 512 cycles. Beyond this frequency, errors increased more rapidly from 270 at 512 cycles to 325 at 4096 cycles. It is to be noted that the complex noise especially rich in high frequencies at comparable loudness levels affected production more adversely than the pure tones.

In evaluating this study one wonders to what degree the increase in errors both with increases in frequency and loudness might be due to progressive dulling of motivation. In spite of efforts to keep motivation constant the high error counts occurred after several days work with high level and high frequencies. On the other hand, one might have expected a gradual adaptation to the noise such as Harmon (24) found, in which case errors would have been least numerous toward the end. It is regrettable, though understandable in view of the lengthy demands on the subjects, that the study was not repeated using the noise intensities and frequencies in reverse order.

Pollock and Bartlett (44) conducted a series of experiments using loud clicks and a complex noise delivered to head phones worn by the subjects while engaged in a simple eye-hand coordination task of picking up and replacing pegs in a moving peg board apparatus. The results indicated that the noise adversely affected performance initially but

adaptation set in rather quickly especially when the task was of such a nature as to permit thorough automatization. Considering long work spells of eight hours the difference in performance between noise and quiet was insignificant. In the case of mental work (word-making) noise, especially discontinuous complex tones, had an adverse effect on speed which was most marked early and late in a working spell of thirty minutes. Again, however, a plan of work, rendering performance more automatic tended to counteract adverse effects. No data are presented of the loudness or frequency of noises used, but performances appeared most affected by the arhythmical character and interestingness of the distractions.

Weston and Adams (57), in England examined the output of weavers over a period of twenty-six weeks during which the workers wore ear defenders on alternate weeks, thus reducing noise from 96 db. to 87 db. as measured by the Barkhausen audiometer (comparable to the Western Electric 3-A, although the reference level for the db. scale is not available) output was one per cent greater while wearing the ear defenders. However, output was largely controlled by loom speeds. The operations by the weavers occupied about five minutes per hour without ear defenders, but with ear defenders were reduced to four and one-half minutes, a speed increase of about twelve per cent. There was some evidence based on hourly records that even after years of work in a noisy environment, the worker does not become completely adapted to noise but goes through the adaptation process daily.

Since the weather in the above study favored work on weeks when ear defenders were worn the same investigators returned to the same weaving shed to repeat their observations over a twelve-month period using two groups of 10 weavers each equated by supervisor's ratings. They discovered a three and one-half per cent advantage in output and a thirteen and one-half per cent improvement in personal efficiency in the group wearing ear defenders which reduced the noise from 96 to 81 db.

The significance of these experiments lies in the apparent demonstration that workers long accustomed to noise, and by no means disposed to complain of it, may show substantial increases in working efficiency as a result of noise reduction. Such a finding is obviously of economic importance in the manufacturing field. The investigation, therefore, merits careful critical consideration.

The second experiment is open to the serious objection that the exact equality in production of the experimental and control groups was never demonstrated. The unreliability of the rating method might have pro-

duced a chance superiority for the experimental groups. It is regrettable that the investigators did not interchange the experimental and control groups.

The influence of suggestion was not controlled. How important a factor suggestion may be can only be surmised. Baker (3) demonstrated with college students working for 5 minute periods under noise and quiet conditions that output can be lowered or raised depending upon subtle suggestions incorporated in the pre-experimental instructions. We have no way of knowing what suggestions were given workers in the Weston and Adams studies. However, since neither the work, nor the length of the work spells in Baker's study are comparable to the industrial situation there is no reason to expect suggestion would be a major factor influencing personal efficiency.

Finally, the variability of percentage improvement in personal efficiency from month to month ranging from 31.42% to 3.42% suggests powerful uncontrolled factors influencing the differences between the groups.

There remains, none the less, the fact of a consistent superiority of output under sound reduction. The authors' conclusions that "excessive noise is a factor having an important effect upon industrial efficiency" and that "tolerance is not acquired with lapse of time" are not irrefutably established, but the weight of evidence is in their favor.

A study by Kornhauser (28) attempted to relate the output of typists to noise in a regular office situation. The results were equivocal and complicated by the existence of other distractors in the quieter location.

Using only two subjects, presumably the authors, Vernon and Warner (54), reported that performance on an arithmetic test, a purely routine assembly job, and reading difficult material were not significantly affected by noise after a short adjustment period during which there were adverse effects. On the other hand judgments of the magnitude of subjective disturbance indicated less rapid adjustment to the noise. These conclusions were based not only upon observations under laboratory conditions but included tests made in the presence of factory and office noises.

In reviewing these reports dealing with production and accuracy of work one is impressed with the methodological difficulties of conducting meaningful investigations "in the field." Other conditions beyond experimental control, such as temperature, machine breakdowns, or changes in quality of raw materials may obscure whatever benefits noise reduction may contribute. It seems clear that a thorough study of industrial pro-

duction as contrasted with office work has yet to be done under positive control of all factors, covering work periods of sufficient length with noise typical of industrial situations. Furthermore, the factor of suggestion in the studies so far published has received little attention possibly because it is difficult to control.

Influences on Vital Processes

The effect of noise on blood pressure, respiration rate, metabolism, muscular tension, electrical skin resistance, digestive processes and similar functions has attracted some experimental investigation. Among the most widely quoted of these is the report by Laird (29) who measured the metabolic rate of four typists during a half hour of resting and while typing a standard letter over and over again for two hours per day throughout a four-week period. During the first and last of these weeks the walls of the testroom were bare. During the second and third weeks sound absorbing material was applied to walls and ceiling reducing the noise produced mechanically from 50 "sensation units" to 40 "sensation units."

On the average the metabolic rate was fifty-one per cent higher while working compared with the resting rate during the quieter weeks, and was seventy-one per cent higher during the noisy weeks. A suggestion that the noisy phase was more fatiguing is found in the data showing the average time for the last five letters of the two-hour spell was seven seconds less than for the first five letters in the quiet phase, while the comparable time was five seconds more in the noisier condition. However, the fast typists improved in speed when the noise was reduced, while the slow typists showed little or no change in overall speed.

No day-to-day data were published by Laird, a fact that has been criticized by Harmon (24), who found a very rapid adjustment to noise, using metabolic rate as the principal indicating function and mental arithmetic as the work. Such a task has a much lower muscular component then typing and could not be expected to affect the metabolic rate to a marked degree. It is important to note that on the initial days of noise Harmon found a thirty-seven per cent increase in metabolic rate. During the same days the average number of errors per day was 12 for quiet and 24.3 when working under noise.

Prior to Laird's study Morgan (38) found evidence of similar extra effort in the presence of noise. His subjects engaged in a form of translating letters into numbers by pressing appropriate keys. Respiration was recorded as well as the pressure exerted upon the keys. The noise came from a variety of buzzers, bells and phonograph records. Noise and

quiet periods alternated during the experimental sessions; the ten minute noise period following thirty minutes of quiet. Noise was responsible for greater key pressure being exerted and greater articulation on the part of the subjects—a device spontaneously adopted in an apparent attempt to overcome the distracting effects of the noise. The time per response increased significantly at the beginning of each noise period but subsequently decreased to a point where output was more rapid than during quiet.

Vernon and Warner (54), Freeman (19), and Ford (18) generally confirm the findings of Morgan and Laird so far as an increase in energy expenditure in the presence of noise is concerned.

Davis (9) measured muscle tension in the presence of noise by means of action potentials. His eighteen subjects were required to do nothing except to sit and listen to the noise presented in a series of two-minute intervals separated by two minutes of quiet. Marked increases in tension appeared on the first experimental day at the onset of each noise period. On subsequent days the change became less and less marked so that by the fifth day tension was virtually unchanged when noise was presented.

The effect of noise on digestive processes was examined by Smith and Laird (49) whose four subjects swallowed a rubber balloon which was later inflated in the stomach. Peristaltic contractions were thus recorded by an appropriate pneumatic system operating a writing point on a kymograph. Noise of 80 db. for ten minutes following a twenty minute quiet period produced a thirty-seven per cent decrease in the number of contractions. The recovery period following the noise showed no consistent pattern or change in stomach action. The effects using a 60 db. noise were less marked but of the same nature. The rate at which saliva and gastric juices flowed was also decreased in the presence of noise. These changes would presumably have an adverse effect upon the total digestive process.

Although details of the experiment have not been published in full, Luckiesh (33) reported that a "demonstration visual test" was performed six per cent more quickly in a quiet room than in the presence of the hum from a motor-generator set.

Other observations in the presence of noise of high intensities and short duration have demonstrated increased blood pressure (35) and increased cranial pressure (26). In the later case, however, one wonders to what degree the sound pressure itself may have affected the recording apparatus.

Davis (10) reported a marked decrease in skin-resistance at the onset

of a five-minute noise period. However, the function gradually returned to normal and reached a value before the end of the noise period approximating the value obtained with a control group sitting quietly for ten minutes. The noise level is not reported.

Even though no experimental studies have been reported dealing with effect of noise on emotional control or emotional stability, the reports above indicate physiological changes of the kind associated with emotional disturbances. Many of the changes noted in response to noise are similar to, if not identical with those produced by stimuli exciting fear, anger, and general emotional tension. Throughout much of the published literature (15) are testimonials that cannot be dismissed lightly indicating that high noise levels are the cause of "nervous exhaustion" emotional instability and related conditions.

Adaptation to Noise

In some of the studies previously cited, notably those by Harmon (24), Morgan (38), Ford (18), Davis (10), Pollock and Bartlett (44), emphasis has been given to the process of adaptation or adjustment to noise. Morgan found an initial decrement in output followed in some instances by greater-than-normal production. Harmon reported an initial increase in metabolic rate followed by smaller and smaller increments on succeeding days at the onset of noise. Pollock and Bartlett's subjects reported they could hardly bear the noise in the early experimental sessions yet at the end they did not notice the noise. These reports do not fit with the long term studies by Laird, Aetna Life Insurance Company, Weston and Adams, which show a net deleterious effect of noise. It is possible that the differences in results can be attributed, in part, to the factor of suggestion as already mentioned in the previous section. On the other hand it is not reasonable to expect that a higher-than-average efficiency index could be maintained without a break over a full year of operation in a group of fifteen clerks by suggestion alone, as the report of the Aetna study shows. It is also true that the studies showing an adjustment to noise such that the detrimental effects are reduced or eliminated cover, for the most part, short noise periods. We may hazard the guess that if the noise were continued over longer periods comparable to those experienced in industry the adjustment mechanism compensating quickly for the initial ill effects may break down and the net effect of noise even in later studies might have been adverse. In line with this hypothesis is Freeman's (19) idea that the added effort noted in the presence of the initial noise is not just a rise in total energy expenditure but comes from a shift in the

pattern of supporting processes, not directly concerned with the principal functions (typing movement, etc.) under observation. With continued exposure to the noise this irradiation of excess tension becomes less prominent and a more economical concentration of supporting processes reasserts itself.

Factors Affecting the Annoyance of Noise

Throughout the studies mentioned above are frequent references to the erratic manner in which noise acts as a distractor. Only a few studies have been reported which were aimed at discovering the features of the noise which contributed to annoyance. Using a paired-comparison method with pure tones at 50 db., Laird and Coye (31) found the annoyance of tones to be ranked in the following descending order: 8192, 4096, 64, 2048; with 128, 256, and 1024 of minimum and equal annoyance. Thus, other things being equal, high tones and extremely low tones are judged more annoying then those in the middle range. The effect of intensity was examined by matching in annoyance a 256 cycle tone at various intensities with other pure tones adjustable in loudness. The equal annoyance contours thus established below 500 cycles followed roughly the equal loudness contours shown in Figure 1. Above that frequency the equal annoyance contours tended to drop away from the equal loudness contours, indicating that low tones of high intensity are as annoying as high tones of relatively low intensity.

The observations of acoustical engineers (40) testify to the importance of high frequencies as they affect the reported comfort of treated areas. In one case two adjacent offices were acoustically treated, but with different materials both having the same noise level and similar sound sources. The absorption coefficient at 4096 cycles for Office A was 0.45 and for Office B was 0.82. B was rated by its occupants as comfortable while those working in A complained that the typewriter noise was not properly quieted. Replacing the lower coefficient material by the other type corrected the complaint, in spite of the fact that the overall Noise Reduction Coefficients for the two materials were approximately the same.

Interrupted noise or discontinuous tones have been generally found to be more annoying than steady noises. This was true in the reports by Pollack and Bartlett (44), and Laird (24).

Furthermore Finch and Culler (13) found greater auditory destruction in dogs subjected to high intensities when the tones were interrupted. Even the reports of Morgan (38), Davis (10), Freeman (19),

Harmon (24), and others who have used relatively short noise exposure periods would lead one to expect that discontinuous noise would be the more detrimental. In all of these reports the initial onset of noise produced the greatest adverse effect. Hence the discontinuous noise would be somewhat comparable to a rapid series of noise periods each of which demands a new adjustment with initial phases of maximum cost to the individual.

A number of writers have referred to a miscellaneous list of factors that influence the annoyance of noise, none of which have been subjected to systematic investigation. Among these factors are the unexpectedness of noise (42), the spreading effect and reverberation (47) the degree to which the noise is unnecessary or indicates malfunctioning of equipment (2).

Of primary interest to psychologists are the individual differences in noise tolerance. Numerous independent observers in the field (47) as well as nearly all laboratory investigations agree in reporting wide differences in the degree to which persons are affected by the same noises. Related to this factor is the wide variation in response by a given person on different occasions. Shifts of attitude, motivation, and attention result in widely different types of performance under the same external noise conditions. No systematic attack has been made on this problem to discover what personality feature or other psychological factors are correlated with noise tolerance.

Stimulation Deafness

Kemp (25) has recently reviewed the studies dealing with stimulation deafness. It is, therefore, unnecessary to review that area except to reemphasize the necessity of interpreting the reported incidence of alleged occupational deafness in the light of the normal decrement in acuity with age. The most complete information dealing with incidence of hearing losses in the general population is that gathered by the Bell Telephone Laboratories at the New York and San Francisco Fairs in 1939 (50). In spite of this precaution which prevents the acceptance of many reports at their face value, there is little doubt that extended exposure to intense noise will produce both a temporary and permanent loss in auditory acuity.

Much of the experimental work in this area has been done on animals in an effort to throw light on the mechanism of hearing. The results have shown in general that the frequencies suffering the greatest impairment are not correlated with the predominant exposure fre-

quencies, although the degree of impairment is related to the duration and intensity of the exposure noises (8, 43, 46, 55).

Although numerous reports are available purporting to show that workers in high noise levels have a greater-than-average incidence of hearing loss (22, 34, 35, 36, 37, 40, 45, 52, 53), the devices for measuring hearing have varied widely making any comparison between occupations hazardous. One survey (61) has been published of nine plants in New York State conducted in a standard manner using a Western Electric 3-A audiometer. The report concludes that the greatest percentage of deaf people occurs in plants of greatest noise. However, it must be pointed out that not all employees in the plants of greatest noise were tested. It is possible that a selective factor operated to exaggerate the proportion of deaf people tested in those plants.

There appears to be no reasonable doubt that high noise levels can and do produce hearing defects especially after long exposure. Still unanswered are the questions, what levels can be tolerated, and for how long without incurring a defect?

Conclusion

In spite of a wide spread interest in noise abatement relatively few facts have been well established. Popular literature not covered in this review abounds in emotional outbursts against the baneful effects of noise. Public support has been enlisted for noise abatement campaigns on the uncritical acceptance of the assumption that noise because it is annoying must be harmful. The available scientifically controlled studies are not in complete agreement but tend to show ill effects in output, speed of work or vital processes. Although a considerable degree of adaptation takes place, the evidence suggests that it is seldom complete. Marked individual differences in susceptibility to the ill effects of noise have been noted but no reported attempts have been made to correlate these differences with other facets of personality. The factors determining annoyance have not been subjected to thorough analysis. Stimulation deafness is an unquestioned result of exposure to loud noises for long periods but its extent and the critical levels of noise necessary to produce it in humans have not been clearly established. In summary, it is clear that there are many circumstances under which noise detracts from efficiency and well-being. Under what circumstances noise is deleterious and for what kinds of people are questions for further fruitful research.

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STUDIES IN TIME PERCEPTION

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The study of time takes various forms and presents problems to investigators in many fields. For example, the nature of time is of special interest to the philosopher. The accurate measurement of time is important for the physicist and the astronomer. In a somewhat related sense, time is a very practical problem for the daily worker who punches a time clock, the athlete who competes in games and the aviator in his maneuvers. The perception of time is a subject of special interest to the psychologist.

In this paper we shall be interested in time perception and time estimation and shall be concerned with such problems as the nature of time only in so far as it is necessary for a definition of our problem. In this quest we will not be greatly aided by such definitions of time as that proposed by Kant who said that time is "a universal form of intellect" or by Leibnitz who described it as "the obscure and confused picture of the grounds which determine the order of succession."

The physicist's definition of time, as the measurement of the movement of the earth through space, is more helpful but it is not entirely satisfactory for our purposes. Neither is the somewhat naive operational definition of the psychologist who says that time is what the clock measures, much more helpful. This is true for the double reason that time existed and no doubt was perceived before the advent of clocks and secondly, because when we ask what it is that the clock measures we are back to our original question.

Such considerations in the absence of any more constructive material leads us to the tentative description of time as our perception of one of the two essential characteristics of movement. One of the essential characteristics is space and the other is time. This brings out the close relationship between time and space. Such an explanation smacks strongly of the theory of relativity and fits nicely with Gestalt theory in psychology. However, this need not necessarily constitute a criticism of the definition. Neither does it inevitably commit us to a purely Gestalt explanation of time since such time-space relations may be assumed to be the result of our experiences rather than nativistically given.

If we wish a more specific definition of time it may be described as the perception or probably better stated as the judgment of the amount of separation of two units or limits as greater or smaller, in terms of

some unit such as seconds. This judgment is reached from an estimation of how long it would take to accomplish so much or so many things such as to walk a block, listen to so much or to feel rested or fatigued by so much work.

While many theories have been proposed and many experiments conducted on the subject of time perception or time estimation* as yet there is no generally accepted view as to how we perceive or estimate time. Studies in time estimation might well be classified under several related but somewhat different problems such as,—

1. The estimation of relatively short time intervals. Most of the experimental work in time has dealt with the estimation of time between two signals up to a few minutes in length. In most cases the estimator has been forewarned that an estimation was to be made.
2. The estimation of time when the subject is not expecting to make such estimation. For example, if the reader was suddenly asked to estimate how long had been spent in reading this paper up to this point, we do not know whether the same methods of estimation would be used as in case he had expected to make this estimation.
3. It is also not certain whether the same methods are used in estimating and in reproducing time intervals. In estimating time a unit of estimate such as a second is necessary. In reproducing time, a unit is at least not so necessary.
4. The perception of longer intervals involving hours or days may be similar or may be different from estimating shorter periods of time. Certainly some of the same factors are involved but some new ones may also be involved.
5. There also is the question of how lower animals perceive time. Do they use the same methods as humans? At least there certainly is little "mental content" used in their estimations.
6. Then there is the problem of estimating the time of day or night. This at least in modern society involves direct reference to some instrument of time measurement such as the clock. In primitive society the sun or moon was, no doubt, generally used as the reference point. That our modern methods are only a refinement of this method is indicated by our usual methods of indicating time by so many hours, minutes and seconds since noon or midnight.

In the following sections of this paper we shall review the principal recent studies that have dealt with the problems listed above. Since, research has not proceeded as logically as the outline, this review will follow the various lines of research.

Since there have been excellent reviews of the literature on time estimation in a series of articles by Dunlap (13) from 1911 to 1916 and a later review by Weber (56) in 1933, this article will in general be limited to the literature since the later date. There will, however, be occasional

* The phrases perception of time and estimation of time are here used somewhat interchangeably. In a sense they are not strictly synonymous. Time estimation always implies a kind of quantification not necessarily included in the time perception.

violations of this principle (1) in order to emphasize some point of view developed during the earlier period and (2) in order to summarize studies in time perception in animals. This later topic was not included in the earlier reviews and it may well have an important bearing on research in time perception.

Cues for Time Perception

One of the important problems in time perception is that of the cues used. In fact, most of the experimental studies in time perception can be related more or less directly to this problem. In the closely allied field of space perception the principal cues as well as their relative importance, as Carr (8) has so well pointed out, have been very carefully investigated. Although many studies have been conducted, no such list of cues has been discovered for time perception. In fact, almost the opposite is true. That is, we are hardly sure of any of the cues that are used in time perception.

Several of the earlier studies reviewed by Weber (56) investigated the relationship between certain physiological processes such as breathing, fatigue or digestive processes and time estimation. Some of these studies found evidence for such relationships but the evidence was never conclusive.

In some recent studies in this field Rosenzweig and Koht (45) compared time estimation and what they called "need tension." They defined need tension as a state of strain. They found that during periods of greater need tension there was more underestimation that when the tension was not so great.

A somewhat similar point of view was presented earlier by Francois (17, 18) and later by Hoaglund (30) who believed that time is mediated by internal temperature fluctuations. For example, Francois found that when the internal temperature was increased by high frequency currents, the tapping rate of the subject was increased. From such kinds of studies these two writers believe we have some kind of internal time clock for the perception of time.

Kawasima (37) had subjects estimate time while moving their arms in ten degree circles. Overestimation occurred when the action was easy and underestimation when it was difficult.

Shaefer and Gilliland (48) systematically varied (1) pulse rate (2) heart work (3) blood pressure (4) breathing rate and (5) lung work, in order to determine the effects of each on the estimation of time. No relationships were found between any of these variables and time estimation. Even when all these changes occurred together producing a

state of great physiological activity they seemed to have no influence on the direction of error or accuracy of time estimation.

Berman (5) investigated the relation of time estimation to what he called satiation. A stylus maze was used to produce satiation. When the subjects were satiated, 87 per cent of his subjects underestimated the time required for them to become satiated while 52 per cent of the non-satiated group overestimated the time required to reach their criterion of learning.

Jasper and Shogass (34) found that the alpha rhythm of the brain was in no way related to conscious estimates of time.

Certain drugs and disease affect the estimation of time. It is common knowledge that the drunk man is generally disoriented as to time as well as space.

Favilli (15) states that under mescal intoxication time estimation is very poor. Forty-five minutes was estimated as 15. But sometimes the estimates are abnormally short and sometimes abnormally long. Favilli explains this by suggesting that in the intoxicated state the shortening of the "field of consciousness" results in a sort of contemplation of the present instant which lacks a frame of reference. This seems to be a way of saying that the subject is always living for the present and lacks the usual perspectives for estimating time. If this be true, mescal intoxication may not have a greatly different effect upon time estimation than alcoholic intoxication. Marijuana also seems to produce a somewhat similar effect. It is sometimes smoked in cigarettes by orchestra leaders and drummers with the idea that it assists them in rapid timing. Whether it does or not seems not to have been investigated experimentally.

Sterzinger (49, 50) gave subjects several different drugs for the purpose of determining their effects on time estimation. After taking quinine subjects habitually underestimated 5 minute intervals. Alcohol produced too low an estimate for periods less than 25 minutes and too high for longer periods. Caffeine and thyroxin gave inconsistent results. Sterzinger came to the conclusion that time estimation depended upon metabolic rate.

Filled and Unfilled Time

One of the early topics to receive experimental study in the field of time estimation was the effect of filled and unfilled time. That is, what effect does the type or amount of mental content or physical activity have upon its apparent length? Although the literature of this topic is extensive the results, as will be seen from this survey, are by no means

consistent. Some results seem to indicate as William James (35) suggested that filled time while passing is perceived as longer than it really is. On the other hand many studies have obtained the opposite results. One explanation for such discrepancies is the fact that it is difficult to determine whether a time interval is filled or empty. The interval may be filled with a large amount of visual, auditory, or other type of external change. However, these may have little or no attention value for the subject who is estimating the time. On the other hand, the time interval may be relatively quiet so far as external stimuli are concerned and still be richly filled with mental content for the subject. For example, he may be thinking, "now one second, now two seconds, three seconds, etc." Many of the seeming inconsistencies in the results for different investigators are due to factors such as those just enumerated.

Much of the early experimental work on filled versus unfilled time was summarized by Triplett in 1931 (52).

Later studies by Helm (29) considered subjective factors such as attention as important in time estimation. Harton (26, 27, 28) found that activities of varying difficulty influenced time estimation. Time spent in making difficult discriminations was estimated as less than equal periods spent in making easy discriminations. In working at tasks in which the subjects were encouraged and felt they were being successful, the time was estimated as shorter than when they were discouraged and felt they were being unsuccessful.

There could hardly be more inconsistency in results than those found in the various studies in estimating filled and unfilled time. Does filled time appear longer or shorter than unfilled time? Does William James' dictum that filled time seems shorter in passing and longer in retrospect hold true? As already suggested, no single answer can be given to this problem except that time estimation depends upon other factors than whether the time is filled or unfilled. In so far as filled time may more often appear longer than unfilled time, it is largely a matter of the subjective filling of the time.

Recent studies by Abbe (1, 2) investigated the effect of increasing the amount of physical space upon time perception. Two blinking lights were used to delimit the time interval. The distance between these lights were varied but the time between their flashes was left constant. Abbe reports that time intervals were reported as longer when the space between the lights was greater. In a related experiment he also found the converse to be true, that is, if space intervals were given in a longer interval of time the space between the lights was judged as longer than when given in a shorter time. Tashiro (51) in a similar experiment in

which he could arbitrarily vary both the rate of tapping and the space between which the taps were made, concludes that the rate of tapping built up a gestalt with the result that the meaning of each tap was lost irrespective of the tapping speed. However, the tempo of the tapping which was determined solely by the spatial distance between the points struck in the taps was more powerful in producing the effect of gestalt movement than the rate of tapping. Rudert-Kotte (**46**) holds that binocular parallax, so important in space perception, may give clues to time perception through the space concept as well as through the slight time variants due to the fact that we see through two eyes instead of one.

While such studies as those just enumerated do not prove conclusively any theory of time perception, they give us material which must be taken into consideration in the formulation of any theory and they strongly indicate that there is a close relationship between the amount of mental content and the estimation of time. Space is directly related to time in that when space is extended this gives the impression of greater mental content. Yet this no doubt is entirely too simple an explanation of time and will need further elaboration as we proceed.

Individual Differences in Time Perception

Three problems relating to time perception will be discussed in this section. One of these is the determination of the indifference point in time estimation. Another deals with the Weber-Fechner law and the other with sex differences in time estimation.

Most of the work on the indifference point in time estimation occurred before 1933 and was reviewed in Weber's (**56**) article. The results may be summarized by saying that different investigators have obtained very different results.

Similar inconsistencies have been obtained in determining whether Weber's law holds in time estimation. Some evidence indicated that errors are proportional to the length of the interval and time studies show no such relationship. In more recent studies Gilliland (**20**) using over 300 subjects with a total of about 5000 estimations ranging in length from 4 to 27 seconds found evidence favoring the Weber-Fechner law. But in another study by Gilliland and Humphreys (**21**) the per cent of errors was found to be significantly greater for the shorter than for the longer intervals. In this study the intervals ranged from 9 to 180 seconds. It seems likely that the difference in results in these later studies was due to the wider range of time intervals in the later study. It seems probable that since time is not mediated by any one sense but through several different senses no single constant could be established

in the Weber-Fechner formula. This may be especially true when the intervals vary greatly in length.

In the earlier studies of sex differences women were generally found to be poorer than men in time estimation. But in later studies such as those by Harton (26, 27, 28) and Gilliland and Humphreys (21) no such differences were found. It seems likely that the modern woman is called upon to make time estimates as often as men. Since time perceptions are primarily dependent upon learning the sexes probably do not differ in ability to estimate time.

Time Perception in Lower Animals

Some knowledge of how time is perceived may be gained from a study of time perception in lower animals. Because we do not believe that the lower animals, at least, use mental imagery to anything like the extent that humans do, any theory that is based upon mental content alone tends to be discredited.

The principal experimental studies of time perception in lower animals may be classified under the general headings of (1) delayed choice response, (2) delayed conditioned responses, and (3) time perception.

There is considerable doubt whether delayed choice responses should be considered under the heading of time perception. The delay of the choice response may not be due to an appreciation of time interval by the animal making the delayed response. Nevertheless, a brief account of such experiments will be included here since the length of the delay is one of the important factors in such reactions.

Hunter (33) using a multiple choice apparatus, measured the time that could elapse between the presentation of a light before the correct choice box and the release of the animal which would result in the animal going to the correct box. In a three choice box, Hunter found that for different rats the time could vary from one to ten seconds but the average time was not much more than one second. For dogs the time averaged about ten seconds and for raccoons it averaged about 20 seconds.

Walton (54) using a different kind of choice box obtained delays up to one minute in dogs. Warden and Warner (55) made tests on the noted dog Fellow and obtained delays of about 20 seconds between spoken commands and their execution. Yarbrough (59) obtained from two to four seconds delay in cats on a three compartmental and 16 to 18 seconds on a two compartment maze. But Corvan (9) obtained delays of 30 seconds with a Persian cat. In contrast with the previous studies Honzik (31) secured delays up to 45 seconds with rats.

The great variance of these results no doubt partly illustrates individual differences in animals but it also illustrates differences in methods used and apparatus. Because of these differences in results no very definite generalizations can be made other than to point out that the higher the animal in the scale, the more likely that the delay can be longer. It is also true that in the lower species the delay is maintained in terms of bodily posture whereas higher animals can change posture and still react correctly after a delay.

Several authors have studied time responses in invertebrates. Verlaine (**53**) taught wasps to discriminate between alleys of different lengths. He thought this showed a "concept of time." Braunschmid (**7**) made studies on time responses of fishes while Grabensberger (**23**) worked with ants and Sterzinger (**49, 50**) with ants, bees, and wasps. Richter (**44**) and Johnson (**36**) have studied the diurnal rhythms of rodents. Johnson found that this rhythm developed in wild mice born and reared in utter darkness. Beling (**4**) working with bees found that he could condition them to return for feeding any time of day or night.

Most of this work shows that some kind of relationship exists between diurnal rhythm, metabolic rate, and some sort of a time sense. But this sense of time is no doubt very different from time perceptions in humans.

Kuroda (**40**) by means of delayed conditioning found that rats were quite accurate up to about 20 minutes and that cats could be given delayed conditioning up to about 30 minutes. Anderson (**3**) experimented on time discrimination in the white rat. He used a multiple choice maze and confined the rat for different periods of time. Rats were able to discriminate between periods of 10 and 20 seconds. Kuo (**39**) constructed a maze with four pathways to the food box. Some were longer than others and in one the rat was confined for 20 seconds. The rats learned to go by the shortest route and avoided the one in which they were confined first of all. These results were further substantiated by an experiment by Sams and Tolman (**47**). Of course, it must be realized that other factors than time perception affect the order of elimination of pathways in such experiments.

Woodrow (**58**) taught two monkeys to differentiate between temporal intervals of 1.5 and 4.5 seconds. They were taught to reach for food upon the raising of a screen after the longer but not after the shorter interval. The monkeys became 90 per cent correct for these time intervals and 75 per cent correct in distinguishing between 1.5 and 2.18 seconds. Metfessel and Bobbitt (**41**) found that canaries could be taught to discriminate and produce rhythms according to a prearranged pat-

tern. They acquired an accuracy with an average deviation of only .01 seconds.

Learning in Time Perception

While some writers claim that the perception of time like other kinds of perception is largely dependent upon certain innate factors, probably no one would deny that learning plays a part in the process. The important questions are how does learning take place and how much of a part does it play. As indicated in the early part of this paper there are probably several different problems relating to time estimation. If so, then there are several problems related to learning in time perception.

The effect of different cultures on the development of time concepts is stressed in two papers. Kloos (38) states that the Persians stress the future while the Chinese stress duration, and the Indians stress the stagnation of time. Rodulescu-Motru (43) believes that the cultural effect amounts to a process of evolution whereby time is evolved through periodic changes in nature, in the body, and in culture. Thus he claims that part of the process of time perception is genetically inherited.

In the field of the experimental study of the time perception in children Elkine (14) reports that in children from ten to sixteen years of age there were large errors in time estimation. He found average errors of 82 per cent for short intervals up to 4.1 seconds and 57 per cent for longer intervals for the younger children. In sixteen year old children the error was 32 per cent for short intervals and 30 per cent for longer intervals. Gilliland and Humphreys (21) found average errors ranging from 32 to 47.5 per cent in fifth grade children as contrasted with errors of 15 to 31 per cent for adults. No doubt the greater accuracy in children in the latter study as contrasted with Elkine's study is due to the fact that Elkine studied Russian children from the home of workers whereas Gilliland and Humphreys studied American children from better homes. Such children have more occasion to be influenced by time and thereby learn the meaning of time intervals at an earlier age than the Russian children.

Several studies have investigated what sense departments are best suited for perceiving time. Goodfellow (22) found that, considering audition as 100 per cent, vision gave 65 per cent and the tactile sense 34. Other sense departments were even lower than any of these. Gault and Goodfellow (19) made two studies; one concerned with discrimination efficiency and the other with reproductive efficiency. In both the auditory sense was most efficient, vision second and touch was third. Forbenius (16) had subjects wake from sleep at previously arranged

times. He reports that they became able to do so within an accuracy of about five minutes. Wirth (57) taught subjects to reproduce given time intervals. Dudycha and Dudycha (11) asked 185 subjects to estimate the amount of time it would take them to perform six different tasks.

Gilliland (20) taught a group of 13 subjects to estimate time by counting. After each estimate they were told the length of the interval. In 25 to 50 practices these subjects were able to reduce their average error from 20 to 25 per cent to 5 to 10 per cent. A similar group with an equal amount of practice but without counting did not improve in their estimating.

Theories of Time Perception

Despite the large amount of disagreement in results in studies on time, many theories have been proposed to explain the perception of time. Most of the older theories postulated some kind of a time sense. The theory presented by Francois (17, 18) and Hoaglund (30) assuming the existence of some kind of an internal chemical time clock comes under this general heading. But later studies have not confirmed this point of view.

Psychoanalysts generally attribute time perception to some phase of the self. Hugenholz (32) explains time as follows: "The covert human time-form can exist only when there is a consciousness of self." Bonaparte (6) tells us that the child in whom the unconscious dominates has only a vague concept of time as in day dreaming and other forms of imagining. Dooley (10) explains time as an "Ego-defense properly placed in that class of defenses comprised of attachments to real objects in counterdistinction to narcissistic attachments." Just what is meant by such explanations and how convincing they are, is left to the reader.

Obendorf (42) explains time as "primarily dependent upon the reality with which recurrent physiological reflexes are registered. It becomes distorted in the absence of the sense of reality and in cases of profound interference with the perception of reality time sense may cease to exist. Another factor which is strongly influential upon time perception is the presence or absence of purpose, for without purpose the value of time greatly diminishes." Sterzinger (49, 50) as has already been stated believes time perceptions are based upon body metabolism, when metabolism is high, time passes quickly, when it is low, time drags. This bears a close relationship to the theory of Francois and Hoaglund and is essentially the theory proposed by du Nouy (12). Dunlap (13) stresses the relationship between time perception and bodily rhythm.

A number of writers emphasize the Gestalt point of view in time per-

ception. Weber (56) stresses the relationship between time and space. In a series of experiments by a number of *Gestalt* writers the close relationship between time and space is emphasized. This relationship is expressed by the formula "phenomenal velocity equals phenomenal space divided by phenomenal time or $v = s/t$." This is further emphasized in the statement "these variations in the flow of phenomenal time are not occasional examples which may be explained as illusions, but that they are continuous and are conditioned by almost any change in the structure of the entire field of movement."

While the evidence is either too conflicting or inconclusive to prove or disprove any theory of time perception, an analysis of the data lends support to certain general principles about how time is perceived. May we here sketch briefly some of these?

As has already been stated the evidence does not support a theory of any direct sensory basis for time perception. Unlike space perception and the estimation of distance time is less directly dependent on sense experiences as such. In the case of lower animals a discrimination of longer or shorter intervals and even some vague notion of time are, no doubt, mediated by physiological processes and body rhythm.

In children this vague awareness of time is further amplified by learning units of time such as a second or hour. This knowledge is gained either directly or indirectly from timepieces. The significance of these intervals are interpreted in terms of body tensions and rhythm. As the child gains further experience it learns more about evaluating how much activity can occur in any given time. The sensory cues can come from any sense modality and the resulting activity may be physiological or mental.

In the case of the rat or dog in the delayed conditioned response it is certainly mostly physical. In the case of the reader in estimating how long it has taken him to read this paper his estimate is probably based upon his estimate or knowledge of how many pages he has read and at what rate he reads a page. Likewise if a person knows that he ordinarily walks about five miles per hour when he has walked what he thinks is about three miles he will estimate that he has been walking for 40 minutes. If a subject is not paying attention to what is going on during the interval or if he has no good idea of the amount that has taken place or of units for measuring the interval, his estimates will be poor. Training as shown in learning the normal rate of one's counting during an interval will soon make a person able to estimate time very accurately. Without some kind of definable content during the interval, estimation remains poor.

In retrospect estimation depends upon the memory of events occurring within any interval. If the interval is filled with many events it will seem long in recall. If it is uneventful it will be remembered as short. Similarly in immediate estimation the estimation will depend upon the amount that has occurred or seemed to have occurred during the interval. It is for this reason that when we are doing interesting things time is generally underestimated. In waiting or boredom time passes slowly. However, there is always a correction that the estimator may make. If a person knows that time is underestimated during pleasant happenings and overestimated in boredom he may correct or often overcorrect for these influences. It is, no doubt, in large measure because of these facts that the literature on filled and unfilled time is so hard to interpret.

According to this theory time estimation is not nearly so directly given from sense data as in space perception and estimation. Time estimation partakes more of the nature of a judgment rather than a perceptual process. The fact that in space perception the cues remain for further examination whereas in time perception after the event it is always in retrospect, helps to make time estimation more of a process of judgment than of perception.

What has thus far been presented as a brief theory of time perception applies particularly to short intervals. Possibly longer intervals are estimated in somewhat the same way but more often with greater reference to external events such as the position of the sun and familiar events such as the amount of traffic on the street as in estimating the time of day. Of course, when a timepiece is available it will be used.

In an earlier part of this paper six general fields of research in time perception were indicated. These are areas in which further research could add much to our knowledge about time. In addition to these general fields of research much insight might be gained about the nature of time from a further study of time estimation in animals. The evolution and improvement of time perception both in children and adults would also, no doubt, throw further light on this subject. Such studies would have both practical value in showing how to train persons in time estimation and would extend our knowledge about the psychological nature of time.

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STEINER, LEE R. *Where do people take their troubles?* Boston: Houghton Mifflin, 1945. Pp. xiii+265.

What do we psychologists do when our personal problems become so severe we are ready to jump off a tall building or take other drastic action? More adequate adjustive behavior involves consulting with a respected colleague or a psychiatrist. Mrs. Steiner is concerned with what the great gullible public does with its problems. Her book indicates that a surprisingly large number of otherwise fairly intelligent people consult such purveyors of psychotherapy as newspaper columnists, radio "consultants," marriage or "get acquainted" bureaus, religious faddists, spiritualists, palmists, and numerologists. These "therapists" compensate for their lack of formal training by assuming an air of great self-confidence and something approaching omniscience.

The book begins with a useful discussion of "What is a psychologist?" It is pointed out that there are recognized courses of training preparing people to be psychiatrists, psychiatric social workers, psychoanalysts, and clinical psychologists. Examination of telephone directories and contacts with license bureaus and health departments in big cities showed an almost complete lack of regulation of people dispensing psychotherapy as long as they avoided hypnosis and the treatment of serious mental disorders.

What sorts of therapy and therapist appear in this wide open field? It is easy to discount "graduates" from the "College of Divine Metaphysics." This institution confers a "Doctor Degree in Psychology" on the basis of two courses, costing fifty dollars each. A surprising thing is the large number of similar institutions giving "degrees." A common characteristic of most of the "therapists" discussed by Mrs. Steiner is their lack even of one of these dubious degrees—one simply adds to one's name a set of letters like Ms.D. or Ps.D. After all, why bother to go to school when your practice is based on common sense, of which you clearly have more than your share?

As a circulation-builder the newspaper columnist-consultant is probably useful to the publisher. However, the advice given may often be harmful and is rarely helpful. Mrs. Steiner sent a copy of the same letter to a number of leading columnists, briefly outlining a mother's problem with a disobedient child. One advocated sending the boy to a special school, and several others proposed stern discipline of one sort or another. Similarly, advice based on inadequate evidence is freely given on a number of radio programs listened to by millions. Both types of "advisors" have in common a certain incompetence and an engrossing interest in the commercial aspects of their ventures.

The alleged therapist who deals with an office clientele is most ob-

viously the profiteer on others' emotional problems. Mrs. Steiner devotes the greater part of her book to a carefully documented report of her experiences with these people and their "patients." We can visit with her the "Human Audit Bureau" where we are given vocational guidance on the basis of our brain development as measured with special calipers. If we need to find companionship of a sort, we can always contact a marriage broker or some introduction service. After all, "Why let lonesomeness rob you of romance, happiness, and comfort?" What shall we do if we are really interested in religion but are not satisfied with the regular recognized churches because they don't seem to solve our personal difficulties? This problem is "easy." There are many local "churches" to take care of us—would we prefer spiritualism mixed with a little hypnosis, or stick with the "I Am" system? Perhaps we are prejudiced in favor of something like palmistry, graphology, astrology, numerology, or tarot; many practitioners of these occult sciences parade through the chapters self-confidently if somewhat denuded of respectability.

Mrs. Steiner is not one of the smoothest of writers. This reader's sensation was often one of bumping along from one section to another with little help in transition. The overwhelming mass of apparently scrupulously recorded and reported experiences making up the book's documentation probably contributes to this feeling of disjointedness. However, the subject material of the book will be of great intrinsic interest to any professionally-minded psychologist, and a careful reading will repay anyone with even the glimmerings of a "social conscience."

A review would be incomplete without at least a mention of some of the book's obvious possibilities for professional use. One could:

1. Recommend it to his more intelligent students in elementary or clinical psychology courses as supplementary reading material.

2. Pass it along as encouragement or reminder to committees in professional organizations engaged in formulation and acceptance of standards to be met by practicing applied psychologists.

3. Use it to strike lightly those psychologists who have obstructed the setting up of such professional standards.

4. Bring it to the attention of legislators and point out similar conditions existing in their own constituencies, hoping that legislative support may thus be obtained for regulation of people unwarrantedly calling themselves psychologists.

5. Write letters quoting its conclusions to newspapers and periodicals accepting advertisements from individuals and organizations similar to those discussed.

6. Pass along the information from it in any ethical, practical way to the public which has so often been victimized.

7. Use of an occasional rereading of sections of the book to reawaken one's

conscience and remind oneself that such abuses do exist, and that it is the professional psychologist's duty to eliminate them.

ROBERT AMMONS.

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KAPLAN, OSCAR J. (Ed.), *Mental disorders in later life.* Stanford, Calif.: Stanford Univ. Press, 1945. Pp. vii+436.

The disparity between the available research on the early segment of development (childhood), and the late (senescence), has long been obvious. The paucity of factual material available on mental decline has no doubt been disquieting to the teacher of developmental psychology who strove to present consistently the entire sequence. Particularly is this void conspicuous when aging is viewed as the ultimate consequence of maturation and integration. Moreover, this period of the individual's growth assumes increased importance since more persons now live to be old, thus steadily increasing the ratio of old people to those in other age brackets.

The editor modestly presents the seventeen chapters as a collection of essays and not "a full summary of the vast literature already available in this field." Although the title and general orientation of the volume emphasizes mental disorders, several of the chapters are good, up-to-date reviews of typical changes with aging.

In these days of specialism it is heartening to find men of diverse scientific backgrounds collaborating on the treatment of phenomena of a limited field, even though this cooperative effort is additive rather than integrative. A brief, general introduction of the volume by a psychiatrist (Karl M. Bowman) is followed by a review of later life disorders by a statistician (Horatio M. Pollock). Well documented systematic articles on the physiological (Nathan W. Shock) and psychological (Harold E. Jones and Oscar J. Kaplan) aspects of normal and abnormal age changes follow. To complete the survey of older age processes the newer and highly significant sociological (H. Warren Dunham) and nutritional (L. Erwin Wexberg) approaches are added.

Chapters on the *Neuroses* (Norman Cameron), the *Involutional Psychoses* (Eugene Davidoff), the *Presenile Dementias* (George A. Jervis), the *Senile Psychoses and Psychoses with Cerebral Arteriosclerosis* (David Rothschild), the *Aged Subnormal* (Oscar J. Kaplan), and the *Toxic Delirious Reactions* (G. Wilse Robinson, Jr.), outline the clinical disorders of this age period with an emphasis on the recent literature. *Psychotherapy* (Fred V. Rockwell) and *Mental Hygiene* (Nolan D. C. Lewis) each merit a chapter in addition to sections on treatment in the clinical chapters. A systematic chapter ends the collection. It is written by a gerontologist (Edward J. Stieglitz) who has a broad perspective

on the phenomena of aging and its unsolved problems. The chapter does not, however, specifically articulate with those preceding it. Sandwiched between the clinical types is a novel chapter describing a study in one institution of *Older mental patients after long hospitalization* (Eugenia Hanfmann).

Like most volumes of this kind, the style, general outlines, extent of organization of facts and completeness of coverage of the materials vary greatly. The editor does not state the degree to which each writer was cognizant of the other contributors' outlines or manuscripts. However, the overlapping and repetitions that occur do not detract from the book.

The contributors differ in the extent to which they deal in mere description and in hypothetical explanation. Some approach their phenomena from an atomic and some from a multiphasic standpoint. Wexberg, in treating nutrition, finds it necessary to discuss employability of the aged. In a discussion of neuroses, Cameron generalizes more as to the basic nature of psychological changes than do the authors of the chapter devoted to that subject. Shock integrates his detailed, scholarly chapter, based upon 281 titles, and attempts to explain impairment of mental functions by the hypothesis that aging is attributed to progressive loss in the homeostatic capacities of the organism. Dunham, with less data, presents a framework to interpret the interplay of culture upon the biological factors.

This factual volume is a distinct contribution to abnormal and developmental psychology. Dr. Kaplan is to be commended for his broad perspective of the problem, his appeal to specialists for data, and his excellent choice of contributors. However, with full cognizance of the enormity of the task and the potential vulnerability of the author, one does regret that a chapter has not been added to attempt to integrate all of the contributions.

FRED MCKINNEY.

University of Missouri.

SHERMAN, M. *Intelligence and its deviations.* New York: Ronald Press, 1945. Pp. x+286.

The first part of this brief text describes the nature of intelligence, mental growth, and the relation of mental growth to physical and environmental factors. There follow brief discussions of intelligence in delinquency and psychosis, but most of the discussion of deviations concerns mental deficiency in its association with neurological disorders. There are brief sketches of intelligence tests, of genetic problems in intelligence, and of intellectual superiority.

The treatment of the topics is very uneven. The author seems at home in the medical discussion of such conditions as cretinism, microcephaly, birth injury, epilepsy and hydrocephalus, and the information,

presented rather technically, will be of interest to the psychologist. There is a chapter on the adjustment of the mental defective which, while short, surveys a number of studies. As a general text, however, this book is not satisfactory.

In a general discussion of intelligence and its deviations, which it is the author's stated intention to provide, one would expect a well-rounded development of the many facets that the subject possesses. Yet, in this very brief book, there is a very meagre treatment of most of the topics, and some important subjects are not mentioned at all. One feels that Professor Sherman wished to present the medical and neurological aspects of certain conditions which occur in some defectives, and then added material deemed necessary to prevent the book's having a rather limited and specialized appeal.

There is a very brief and inadequate discussion of the rationale of intelligence testing, a knowledge of which is indispensable to the systematic understanding of the field. There is very little mention of qualitative features of test performance, test patterns, problems of rapport, differences between diagnostic groups (some of which are never mentioned), or of many other significant and important topics. From the book, one would conclude that the only really significant deviation from normal intelligence is in the direction of deficiency. Seven pages are devoted to the intellectually gifted! In the chapter on intelligence and delinquency, the major problem raised is whether the average intelligence of delinquent groups is below that of other groups. Only eight pages are devoted to intellectual changes in psychoses other than the psychoses of mental defectives, and little of the extensive literature in this area is cited. Few of the basic problems studied are made known. It is a sterile and rather futile picture of the study of intellectual deviation that is revealed in these pages. None of the studies of intellectual function of such investigators as Goldstein, Hanfmann and Kasanin, N. Cameron, J. Hunt (to mention but a few) is mentioned, and the whole problem of test performance in relation to lobectomy is ignored.

The emphasis in presentation is well illustrated in the chapter on epilepsy, to which 19 pages are devoted. Three pages are given to intellectual changes (deterioration). The nature of epilepsy, its age of onset, personality changes, and its pathology are described, and one derives a reasonably adequate picture of the disease. But this is a book about *intelligence* and *its* deviations.

The discussions of general topics, such as theories, growth, and the like, are poorly organized and will hardly leave the student with a clear idea as to what the problems are or what has been done in their solution. Professor Sherman feels, for example, that IQ differences found in identical twins reared separately are an indication of "achievement levels" dependent upon different environments. This, of course, is the crux of the matter. But he then argues that such results cannot evi-

dence the influence of environment on intelligence, because tests are not culture free (253). Is the concept of intelligence itself culture free?

There are a glossary of terms, mostly medical, and a bibliography of over 300 titles. It is perhaps significant that but seven titles bear publication dates later than 1939.

CHARLES N. COFER.

George Washington University.

BRENNAN, ROBERT E. *History of psychology, from the standpoint of a Thomist.* New York: Macmillan, 1945. Pp. xvi+277.

The author presents and evaluates the history of psychology from the point of view of Thomas Acquinas, whose views in turn were based upon the writings of Aristotle. Nearly half of this condensed history is devoted to the antecedents, philosophic and otherwise, of psychology as a separate field. The remainder of the book is devoted to an accurate but very brief presentation of the important events, research, and personalities in the development of modern psychology. The following is evidence of the brevity of the presentation: Wundt, and his achievements, are presented in 4 pages, although there are references to his name or work on 18 other pages; William James is given 3 pages and 4 other references; while Freud is given 13 pages and 9 other references.

Historical sequence is carefully followed *within* the development of a topic, but the omission of dates limits the possibility of temporal cross reference from one topic to another. This is compensated for in part by the inclusion of a bibliographical index which gives the years of birth and death as well as listing the chief publication of the individual by title.

The author's viewpoint is epitomized in the following paragraph,

> What we need today, as Acquinas would contend, is really less of psychology and more of anthropology, using the term "anthropology" in its traditional meaning to signify the study of man, as man, not as a concatenation of reflexes, or a sum of perceptual configurations, or a series of imaginal processes or a complexus of instinctive responses. Such things are simply isolated events in the history of human nature, and they have no meaning except in relation to the whole nature. Further, the study of man, as man, means the study of man as a besouled organism, or as a creature composed of matter and spirit, whose operations fall within the dimensions of scientific analysis, but whose fundamental nature is the proper study of philosophy. (257)

The research man will probably find the material of this book too condensed and the references too limited to serve his interests. Many an academic man will object to the philosophical and religious emphases as being too extensive for his effective use in the classroom. However, considering the religious and philosophic background of the author, most psychological readers will be gratified to observe the emphasis

placed on and the respect shown toward the *established scientific observations* in all fields of psychological investigation.

WILSON MCTEER.

Wayne University.

TRAXLER, A. E. *Techniques of guidance: tests, records, and counseling in a guidance program.* New York: Harper, 1945. Pp. xiv+394.

This comprehensive survey of methods and procedures for use in guiding and counseling high school students is written for the benefit of high school teachers and administrators. The book includes (xiii) "much detailed explanation concerning tests and other instruments of evaluation and a large number of illustrative record forms," written to advise school personnel who may have had little psychological training. Detailed and pertinent suggestions on a variety of associated administrative problems are also provided. Such a book is to be evaluated not as a scientific treatise but as a guide to the proper application of psychological techniques and procedures.

Apart from a brief discussion of occupational opportunities early in the book and an equally brief treatment of "adjustment" problems at the end, the primary concern is with problems of distribution or placement of students within the educational system. The book is organized around the individual cumulative record, which is to include information obtained periodically for each student in the following ten areas: home background, health, out-of-school experiences, plans for the future, school history and record of class work, mental ability or academic aptitude, special aptitudes, achievement and growth in different fields of study, educational and vocational interests, and personality. Information in the first four areas can be obtained from each individual by interview or questionnaire, and school records accumulate more or less automatically.

Information in the last five areas involves the use of tests and observational procedures, and it is here that a detailed explanation of methods of obtaining and applying such information is needed. A minimal explanation of the applicability of a measure in a guidance situation should include a statement of the validity of the measure in predicting scholastic, vocational, or other performance, so presented that a statistically unsophisticated person could grasp the actuarial nature of the problem. This we are nowhere given.

The chapter on the appraisal of aptitudes describes or mentions a number of standard tests of intelligence or general academic aptitude suitable for use with high school students. A carefully prepared annotated list of thirteen such tests, which is presented at the end of the chapter, gives for each test the author(s), publisher, cost per copy, mention of the component parts, and reliability coefficients. For only one of the thirteen tests is validity in relation to scholastic performance

mentioned at all. Since tests of this type do have substantial validity for many purposes, and considerably less validity for many others, it would seem that a more intensive consideration and explanation of this subject would lead to a more discriminating use of such tests.

Under "special aptitudes," similar information is given for tests of art, music, science, mechanical, clerical, mathematics, and foreign language aptitudes. Counselors are warned that the label of a specific aptitude test does not always indicate accurately what the test actually measures, but methods of choosing the most effective of several similarly named tests are not provided.

A chapter on the evaluation of achievement presents an exhaustive annotated list of achievement tests in all scholastic areas.

In presenting a catalogue of personality tests, questionnaires and interest inventories, the author is carefully critical of some of the procedures reviewed; others are simply mentioned or described briefly. (107) "Although there have been many studies of personality tests, the validity of the various tests in this field has not been established." Thus the precautionary statement earlier (100), "Before introducing a new test for measuring personality, a school will do well to try it out experimentally with a few pupils to see whether or not it contributes to the description of the personality of the individual pupil."

Several rating scales are described with a caution about "halo effect." Much more attention is given to the "anecdotal record," a systematic collection of anecdotes "concerning some aspect of pupil behavior which seems significant to the observer." No attempt is made at a statistical evaluation of this anecdotal procedure, either as to inter-observer reliability or as to the prognostic value of various types of behavior samples.

In a separate chapter, a large number of uses to which objective test results may be put are described. A discussion and proposed solution of the validity situation is given as follows (199):

> Research is the only means by which the prognostic nature of a test can be adequately investigated, but research techniques are often slow and expensive and not enough time has elapsed since the publication of the majority of the good tests for research to discover their relationships to fundamental long-time objectives. . . . Personnel workers in schools, however, cannot wait until research with tests has supplied answers to many of their questions, for they must meet today the problems of guiding their present students. Nor is it necessary for them to wait, for they can read a great deal of meaning into the scores on the basis of their own experience.

The reviewer believes that if some of the effort being spent on reading meaning into test results could be channeled into research on their relationships to objectives, the number of areas in which we read meaning *from* rather than *into* scores could be increased.

MERRILL ROFF.

Indiana University.

YALE, J. R. *Frontier thinking in guidance.* Chicago: Science Research Associates, 1945. Pp. 160.

This book is an anthology of 24 articles in the field of guidance. All except one of the papers has appeared elsewhere and are reproduced in this volume because, in the opinion of the editor, they "are selections written fairly recently which the well-informed person should know or know about" and are "recommended reading of definite interest to the guidance person." Each of the 24 contributors (13 of whom are psychologists) is a recognized authority and has made significant studies in this field.

There are six parts to the book: *Part one, Education prepares to increase guidance,* contains a discussion of needed reforms in the secondary school system (Reeves), and presents proposals of the National Resources Planning Board (Blaisdell) for increased federal support and of the New York Regents (Wilson) for increased state support of secondary education; *Part two, Guidance comes of age,* contains articles on the genesis of modern guidance (Paterson), the growth of occupational testing (Borow), the relation between guidance and instruction (Brewer), evaluating the effectiveness of counseling (Wrenn and Darley), the importance of guidance in the elementary school (Addy) and includes a summary of the statement prepared by the ACE Committee on Problems and Plans in Education on student personnel services; *Part three, School guidance programs in operation,* discusses plans developed in Glencoe, Illinois (Kawin), Philadelphia (Boyer) and San Francisco (Schmaelzle); *Part four, Undertaking readjustment of the veteran,* presents material pertinent to the counseling of veterans in the post war period (Williamson), accrediting veterans returning to school (Jacobson) and two states—Connecticut and Michigan—veterans' programs (Gray, Fern, Horn); *Part five, Tools for the guidance worker,* includes articles on non-directive counseling as an effective technique (Rogers), a survey of occupational filing systems (Yale), occupational analysis activities in the War Manpower Commission (Shartle and Dvorak), a critical analysis of textbooks surveying the field of guidance and student personnel (Fredenburgh) and a minimum library for counselors (Smith); and *Part six, The counselor obtains additional training,* contains sections devoted to laboratory training for counselors (Lloyd-Jones), selecting and training teachers—in Fargo, N. D.—for individual guidance (Froehlich) and short term training programs for counselors (Jaqua).

Obviously with such a miscellaneous collection of topics it is difficult to arrive at an adequate overall evaluation. The articles were prepared quite independently of any thought of inclusion in the volume under review, so duplicate each other in minor ways and do not of course constitute a systematic treatment. Certain of the articles are critical in content whereas others are merely descriptive. They are interesting but undoubtedly many similar volumes of completely different articles could have been prepared, so one may ask, why *this* particular collection?

There is no adequate discussion of this point, nor is there any introduction or summary that brings together the points which the editor may have thought significant. It would seem that if the separate articles are worth presentation in a single volume (and the reviewer is not doubting this) they should also be worth an adequate summarization in some chapter designed to integrate the ideas involved. If the title were less pretentious and something like "A few readings in the field of guidance" the contents would be much more accurately described. This is not to deny the editor's contention, however, that some of the articles included do represent significant and critical thinking in the sense which the title implies.

LEONARD W. FERGUSON.

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DUNSMOOR, CLARENCE C. (Chairman) for New York State Counselors Association. *Practical Handbook for counselors.* Chicago: Science Research Associates, 1945. Pp. 160.

Although this handbook was prepared to assist high school counselors, it will be of interest and value to high school teachers and counselors, college and university counselors, teachers college faculty who teach courses in guidance or school administration, and to school administrators or boards who are responsible for authorizing guidance programs and selecting counselors.

An outstanding merit of the handbook is the comprehensiveness of the guidance program which it outlines. It outlines a program extending from the elementary school through follow-up studies of post-school adjustments and including educational and vocational guidance, psychological testing, personal adjustment counseling, study methods, and orientation to the school. Its major deficiency is to be found in the area of actual guidance methods. That is, the handbook presents an excellent outline of the administrative procedures, tools, and techniques of collecting data to be used in guidance but does not go very far in telling the counselor how to use the data in guiding the individual. For example, several psychological tests are listed but very little comment is made on how to interpret or use the test results; several study skills are listed but no suggestions are given as to methods by which they can be developed; and the techniques and content for a case study are outlined but no suggestions made as to how to use or interpret the data collected. It appears that the authors assumed that the counselor who uses the handbook has been trained in guidance methods and psychology but can profit by a comprehensive review of the scope of work that he should include in his guidance program. In spite of this deficiency,

the handbook deserves a distinctive position in the literature regarding guidance programs in the elementary and secondary schools. If a school system organized a guidance program of the scope outlined in this handbook and secured adequately trained personnel to do the guidance work, it would have an excellent program. However, one administrative arrangement the handbook omits is for the counselor to have access to psychological or psychiatric assistance in the diagnosis and treatment of behavior problems, nor does it recommend that the counselor be trained in psychological therapy, abnormal psychology, or mental hygiene.

The handbook contains eighteen chapters, which are very brief, averaging seven pages per chapter. The chapters cover such topics as: the role of the counselor, the objectives, scope, and content of the guidance program, technique of the counseling interview, the maintenance of cumulative records, tests of abilities, achievement, interests, and personality, content and techniques of the case study, group activities as a part of the guidance program, orientation and adjustment to the school, course selection and program planning, vocational guidance, placement program, preparation for college, financing a college education, follow-up studies of post-school adjustments, the counselor's personal qualifications, the counselor's relationships with other persons and agencies, the counselor's tools, and the counselor's professional development.

Chapter 17, the *Counselor's professional tools*, contains an excellent bibliography of books, journals, and other publications dealing with youth problems, guidance techniques, specific phases of guidance, and occupational and educational information. At the end of the handbook is a bibliography of additional books on various phases of guidance and a list of agencies which establish the qualifying requirements for various occupations and professions.

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TRIGGS, FRANCES O. *Personnel work in schools of nursing.* Philadelphia: W. A. Saunders Co., 1945. Pp. xv+237.

The "personnel point of view" is doubtless as applicable and as efficacious in the field of nursing training as in any other area of education. In fact, a very considerable amount of work has been done in this area, as is evidenced by the bibliography of 354 titles included in the present book. The book itself is in the nature of a general survey of the field of student personnel work with particular reference to the problems of student nurses.

The general treatment of the topic is rather eclectic with a generous infusion of the "newer approach to psychotherapy" sponsored by Rogers

and by the Elliotts. The consequences of this eclecticism are evident in a conflict between statistical objectivity and clinical case-work.

The book starts with a sketchy exposition of *Human Behavior and Adjustment* which runs the gamut of physiology (the autonomic nervous system and the endocrines) emotion and motivation, learning and conditioning and the psychology of adjustment—all in 23 pages. This is followed by five chapters on *The Counselling Program* (educational, personal and vocational), and three chapters on testing and personnel records. The final chapter, entitled *The Story of a Student*, sets forth a complete case history in some detail.

The treatment is rather uneven. The sections on counselling problems are written in a style that is clear and straightforward, if somewhat too cursory. In handling the more technical topics of testing and statistical analysis, however, the author often descends to a "popular" level which is confusing at best.

Altogether, the book presents a sympathetic and (for the most part) readable account of the work of the counsellor in a school of nursing which should serve as a stimulus to "start the prospective professional counsellor . . . on the road to her technical training." Its major weakness lies in its failure to provide a rational basis for the predictive process and its reliance on an uncritical amalgam of any and all information arrived at through the somewhat mystical medium of the counselling interview.

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